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ABSTRACT

The focus of the contributions to this journal issue is communication theory and research. Following an introductory article that deals with the need for quality control in the criticism of communication research, eight articles offer discussions of the following topics: the nature of criticism in rhetorical and communicative studies, some issues in evaluating the social significance of communication research, a critical celebration of criticism of communication theory and research, fundamental issues in the criticism of communication theory and research, suggestions for the design of empirical communication studies, the impact of value and self-esteem messages in persuasion, the effects of substantive and affective verbal conflict on the quality of decisions of small problem-solving groups, and a communication perspective on deviation. Two additional articles offer a commentary on Ronald Reagan's attempt to build a national majority in his 1976 campaign for the presidency of the United States and a discussion of political reality and rhetorical criticism. (FL)

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[Thematic Issue: Communication Theory and Research]

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THE CENTRAL STATES SPEECH JOURNAL

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The Nature of Criticism in Rhetorical and Communicative Studies *Karlyn Kohrs Campbell* 4

Criticism is a systematic activity whose structure is defined by and derived from the qualities of the communicative and rhetorical acts which are the objects it examines as well as its medium of expression. To understand the nature of the activity, one must examine it from the perspective of its reflexive, cognitive, dialectical, and evaluative dimensions.

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substantive and nonsubstantive discussions; (3) the significant difference on flexibility in substantive conditions is accounted for by male participants, not women; and (4) no difference on flexibility between affective conditions.

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CRITICISM OF COMMUNICATION RESEARCH: AN INTRODUCTION TO QUALITY CONTROL

Thomas J. Knutson

EVERY year at our professional meetings, various interest groups deliberate over the role of the critic in their respective divisional meetings. Those of us who investigate human communication through the scientific method are no exception. Aggrieved authors attack the system, critics defend their roles, editors and chairpeople advance critical guidelines, and bystanders wonder not infrequently over the rancor with which these debaters spill out into elevators, hotel hallways, and various cocktail parties. Indeed, much of our time is spent arguing over the relative quality of individual research contributions and, for the more concerned or depressed, the value of scientific communication research in general. Whatever these activities do or fail to do, one observation is eminently clear: criticism is inherently exciting. The essays which follow not only capture that excitement, but endeavor to spell out in a more precise form the role and function of criticism in communication theory and research.

Not too long ago, a senior member of our profession was heard to remark, "We have two kinds of people in our department: counters and readers. Readers are better." Today, that comment would not be tolerated, and the speaker would be indicted for a spurious dichotomy. Yet the observation illustrates a condition within the ranks of those who use the scientific method to investigate human communication: We talk with each other too much about science without explaining to non-scientists why we do the things we do. Too frequently, for example, communication critics have been guilty of emphasizing methodological issues at the expense of theoretical concerns. As scientists we can ill afford such disproportionate emphasis on the methodology; we cannot only be counters. Statistical analyses are an integral and important part of science, but as Skinner cogently points out, ". . . they are not science itself."[1] Methodological criticism is worthwhile and valid,[2] but the other important areas of our work require the same critical attention.

Those of us who have chosen science as a tool believe that demonstrable merits and results will issue from our labor. We cannot, however, neglect other aspects of our society and profession upon which our collective success or failure will depend. Procedural and methodological disputes cannot be al-

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[1] B. F. Skinner, *Science and Human Behavior* (New York: Free Press, 1953), p. 12.

[2] For an excellent example of methodological criticism, see Lawrence J. Chase, "Diffusion of Power-related Data in Communication: A Research Note," *Human Communication Research*, 3 (1977), p. 363.

lowed to interfere with attempts to find out more about the complexities of human interactions. In the early days of the battle between rhetoricians and scientists, for example, both groups engaged in behavior similar to that described by Janis as "groupthink."[3] Scholars asserted an almost invulnerable set of advantages of one method over another and repeatedly denegrated other methods of inquiry. These battles have either been resolved or largely forgotten, but an annoying artifact lingers. Too frequently, the communication scientist behaves as the Pharisee in the temple who thanked God that he was not like other men. The scientific critic in many cases comments on scientific research as an elitist making pronouncements upon the elite. That type of behavior wins few friends and still fewer advancements in knowledge about human communication.

The modern scholar in human communication must be both reader and counter, both theoretician and methodologist. In addition, however, the scholar must be a critic in all aspects of research. We have an intellectual and social responsibility to describe our work in a fashion that others can understand and appreciate. We must learn to give more attention to the production, consumption, and communication of our research in a rigorous, critical fashion. The issues become common to all fields of scholarly inquiry: To what extent does the evidence allow the initial interest in the topic? To what extent does the evidence allow for the use of the methodology employed? To what extent does the evidence allow for the advancement of knowledge claims? To what extent does the evidence allow for future developments and refinements? Stated simply, criticism must transcend the reader-counter dichotomy and speak to the generic quality of the research effort.

Criticism is an important part of the modern scientist's activities: Ideally, it provides the scientific community with a means of quality control. Criticism should be employed to appraise the quality and value of our knowledge claims, the accuracy of the procedures and methods employed to arrive at those claims, as well as the underlying merit of the endeavor itself. Unfortunately, however, criticism too often operates in our field as a means of quantity control rather than quality control. The sheer volume of scientific research submitted to editors and convention programs often precludes suggestions for improvements. Critics decide, but they seldom set forth the reasons for their decisions in a way that helps the writer improve the quality of the work.

The need for critical analysis of communication research cannot be disputed: In what way can criticism assist in securing reliable information about communication? Neither can one quarrel over the observation that critical criteria very widely within our profession. We seem to be at a stage reminiscent of forensic arguments fought long ago: there exists a prima facie case for criticism of scholarly work, but the vehicles designed to meet that need have emerged as woefully inadequate. One cannot suppress a feeling of despair, for example, when so many critics decry our collective failure to create communication theory, address important issues, or to raise meaningful questions. In the most recent analysis available, Bochner goes so far as to indict over a decade of

[3] Groupthink refers to a set of conditions which occur in highly cohesive, decision-making groups. Groupthink is characterized by ineffective interaction which leads to an ineffective decision. For a complete explanation of the concept see Irving L. Janis, *Victims of Groupthink: A Psychological Study of Foreign-Policy Decisions and Fiascoes* (Boston: Houghton Mifflin Company, 1972).

research activity as trivial and unproductive.[4] If the situation in fact is as bad as Bochner describes, one can also inquire as to the function of criticism over the past decade. If our research has failed, then surely our criticism has also failed in establishing acceptable standards of communication research. The need for a close consideration of the papers presented in this symposium becomes, therefore, an essential and professional responsibility.

As Ravetz has commented, "The best of all scientific work is that which survives . . . to become genuine scientific knowledge."[5] Of course, that observation requires a post hoc quality control and fails to assist in determining whether any given article will ever be bound in a professional journal. Most scientists write their work sure in the knowledge that if an evaluator, editor, or reader can be convinced, the research will contribute to the development of their field. Indeed, quality control in communication research operates primarily as a critical judgment about the likelihood of any given study surviving. The precision and criteria employed to make this judgment, however, vary widely.

Our field currently operates with an obscure and capricious set of critical criteria. Communication research is an all too typical example of the happy anarchy Ravetz describes as characteristic of the scientific community.[6] No clear set of acceptable standards have been accepted as characteristic of useful criticism. Rejection notices reflect the prevailing mood of the editor, and convention critics frequently operate with reckless abandon and little consistency from program to program. In fact, we operate under a minimum of formality when addressing the worth and contribution of research. There can be no scientific cookie-cutter approach to criticism; however, some modification of the present happy anarchy must be made unless we desire to spend yet another decade running as fast as we can to stay where we are.

The scientific attitude requires a willingness to submit one's ideas to the test of criticism. The critical test in communication research has operated with strange, nebulous, and, in some cases, contradictory components. The test, however, must be made. It is through this critical activity that our field's quality control can be improved. One aspect of critical activity and quality control is illustrated by the papers which follow: our critics must be capable of producing work with a similar degree of quality to that which they evaluate. The authors of these papers clearly meet that initial criterion. Consideration of what they write may well lead to more productive and successful communication research. Regardless of what the nature of criticism may be, however, the importance of the quest requires that all of us continue try to pursue the elusive complexities of human interaction. Perhaps our initiative will not be tampered if we remember the words of the great Finish composer Jean Sibelius, "Pay no attention to what the critics say; there has never been set up a statue in honor of a critic." Given the present morass of critical judgment, there is no reason one should be.

[4] Art Bochner, "On Taking Ourselves Seriously: An Analysis of Some Persistent Problems and Promising Directions in Inter-Personal Research," *Human Communication Research*, 4 (1978), p. 179.

[5] Jerome R. Ravetz, *Scientific Knowledge and Its Social Problems* (New York: Oxford University Press, 1971), p. 275.

[6] *Ibid.*, p. 280.

THE NATURE OF CRITICISM IN RHETORICAL AND COMMUNICATIVE STUDIES

Karlyn Kohrs Campbell

WRITING in 1965 about the meaning of criticism, Edwin Black took the scientist as his model for discerning the essential qualities of critical action.[1] Some thirteen years later, in the forward to the second edition of *Rhetorical Criticism*, Black demurs to emphasize the pivotal role of the critic's subjectivity and style in the critical act.[2] There is no model for critical action except that provided by critics themselves; as Black wrote, "criticism is that which critics do."[3] The activity of critics is the only basis on which to understand the meaning of criticism.

My task in this essay is a difficult one—to articulate anew the meaning of critical activity, and my task is made more difficult by the charge I have been given—to address its meaning, not solely as the humanistic study of rhetorical acts, but as a scholarly activity engaged in by all those who study communicative and rhetorical acts.

The acts of two persons will serve as models for what I take to be criticism. In the book to which I have referred, Edwin Black analyzed the dominant theoretical and critical perspective in rhetoric, defined its limitations and proposed an alternative which he has elaborated theoretically and applied in critiques.[4] In communicative studies, Gerald Miller has proclaimed (as he does once again in this volume) the limitations of certain research procedures, suggested alternative methodologies, and sought to uncover fundamental assumptions underlying theory.[5] Their efforts, along with those of many others, have contributed to the present situation. In rhetoric, a great number of critical essays has been printed analyzing rhetorical acts using a variety of critical approaches and exploring the meaning of alternative perspectives.[6] In

Karlyn Kohrs Campbell is Professor of Speech and Drama at the University of Kansas.

[1] Edwin Black, *Rhetorical Criticism: A Study in Method* (1965; rpt. Madison: Univ. of Wisconsin Press, 1978), 1-9.

[2] Black, x-xv.

[3] Black, 1.

[4] In addition to *Rhetorical Criticism*, see "Frame of Reference in Rhetoric and Fiction," *Rhetoric and Poetic* ed. Donald C. Bryant (Iowa City: Univ. of Iowa Press, 1965), 26-35; "The Second Persona," *Quarterly Journal of Speech*, 56 (April 1970), 109-119; and "The Sentimental Style as Escapism, or The Devil with Dan'l Webster," *Form and Genre: Shaping Rhetorical Action* ed. K. K. Campbell and K. H. Jamieson (Falls Church, Va.: Speech Communication Association, 1978), 75-86.

[5] See, among others, "Theory in Quantitative Speech Research," *Western Speech*, 28 (Winter 1964), 15-22; "On Defining Communication: Another Stab," *Journal of Communication*, 16 (June 1966), 88-98; "A Crucial Problem in Attitude Research," *Quarterly Journal of Speech*, 53 (October 1967), 235-240; "Humanistic and Scientific Approaches to Speech Communication Inquiry: Rivalry, Redundancy, or Rapprochement," *Western Speech*, 39 (Fall 1975), 230-239; with Charles Berger, "On Keeping the Faith in Matters Scientific," *ibid.*, 42 (Winter 1978), 44-57.

[6] A sampling of recent essays from different perspectives includes Ernest Bormann, "Fetch-

the area of communicative theory and research, a spate of essays has appeared reporting the findings generated by new research procedures and outlining alternative theoretical approaches.[7] These changes in the temper of the discipline have not been greeted with universal approval. We may rejoice at the evidence of disciplinary health and growth while being frightened at the plethora of conflicting options. There appears to be no clear path for a rhetorician or a communicologist to follow.

In a situation of flux and tension, those who carry messages we do not wish to hear may be vilified. In all situations, however, critics play a difficult role. Some attack them as cannibals who feed off their own kind; others shrink from them as cold analysts lacking spontaneity and feeling. Some view them as a babble (Babel?) of disputing voices arguing about matters of taste, and many dislike what is perceived as their negative and condemnatory posture. However unflattering, these characterizations of critics have some merit because they reflect inherent elments in critical activity. Critical acts are reflexive, cognitive, dialectical, and evaluative. These qualities reflect and temper the special relationships among the critic, the object of criticism, and the critical act which Black has noted. In what follows, I shall examine critical subjectivity by exploring these four dimensions of critical action.

I shall assume that criticism in rhetoric and communication is an autonomous activity whose essential principles are drawn from the nature of the communicative and rhetorical acts that are studied in this discipline and not from history or psychology or anthropology or any other external field of study.[8] I shall argue that criticism is a systematic activity whose structure is defined by and derived from the qualities of the communicative and rhetorical acts which are the objects it examines as well as its medium of expression. Critical activity in all disciplines shares some of the qualities I shall discuss because all criticism is verbal and symbolic and takes its character, in part, from its medium. However, the claims that I make are limited to and illustrated by criticism as an activity in studies of rhetoric and communication.

The Reflexive Dimension

Even a moment's thought about the

ing Good Out of Evil: A Rhetorical Use of Calamity," *Quarterly Journal of Speech*, 63 (April 1977), 130-139; Michael C. Leff and Gerald P. Mohrmann, "Lincoln at Cooper Union: A Rhetorical Analysis of the Text," *ibid.*, 60 (October 1974), 346-358; Michael McGee, "In Search of 'The People': A Rhetorical Alternative," *ibid.*, 61 (October 1975), 235-249; Michael McGuire, "Mythic Rhetoric in *Mein Kampf*: A Structuralist Critique," *ibid.*, 63 (February 1977), 1-13. An analysis of various perspectives is made by James W. Chesebro and Caroline D. Hamsher, "Contemporary Rhetorical Theory and Criticism: Dimensions of a New Rhetoric," *Speech Monographs*, 42 (November 1975), 311-334.

[7] Jesse G. Delia, "A Consructivist Analysis of the Concept of Credibility," *Quarterly Journal of Speech*, 62 (December 1976), 361-375; "Constructivism and the Study of Human Communication," *ibid.*, 63 (February 1977), 66-83; Leonard C. Hawes, "Elements of a Model for Communication Processes," *ibid.*, 59 (February 1973), 11-21; "How Writing is Used in Talk: A Study of Communicative Logic-in-Use," *ibid.*, 62 (December 1976), 350-360; Gerry Philipsen, "Speaking 'Like a Man' in Teamsterville: Culture Patterns of Role Enactment in an Urban Neighborhood," *ibid.*, 61 (February 1975), 13-22; "Places for Speaking in Teamsterville," *ibid.*, 62 (February 1976), 15-25; two special issues of the *Western Journal of Speech Communication*, 41 (Winter 1977) and 42 (Winter 1978), addressing the question "What criteria should be used to judge the admissibility of evidence to support theoretical propositions regarding communication research?; and two special issues of *Communication Quarterly*, 25 (Winter 1977) and (Summer 1977) that examine rule-governed approaches to the study of communication and naturalistic approaches to the study of communication, respectively.

[8] I am arguing a position analogous to that taken by Northrop Frye, "Polemical Introduction," *Anatomy of Criticism* (New York: Atheneum, 1967), 5-29.

nature of human symbolic activity will reveal that criticism is an inevitable and integral part of it. Human symbolization is distinctive in being reflexive; that is, at the same moment that we are using symbols we are aware of ourselves as symbol users, interpreting our actions, refining our execution, and evaluating the choices we have made.[9] This internal feedback, our continuous metacommunication to ourselves about our own communicative action, is the ground of criticism. As humans using symbols we are ever and always critics. The reflexive or "cannibalistic" dimension of critical action is our symbolic heritage as humans. The irritation we feel toward external critics may, perhaps, mirror our resentment of the internal critic, the true serpent in our communicative Eden, who confounds the desire for total spontaneity or complete transparency.

The character of human symbolization not only makes criticism inevitable and irritating, it also reflects the close relationship that must exist between the object criticized and the critical act. Based on the nature of human symbolization, one might argue that no symbolic act is complete without the critical dimensions, and that only one who could produce the original symbolic act has the credentials to function as its critic. Much has been written about the kind of encounter which must occur between the critic and the object of criticism if the critical act is to be fully realized.

The coming together of the critic and the object, whether it be a speech or essay, a research report, or a theoretical statement, occurs in two stages. The first phase is pre-critical and analytic.[10] Initially, there is a linear or sequential experience of the act as it unfolds that is followed, ideally, by a critical synthesis that apprehends the act as a unity. Rosenfield labels this direct, unmediated encounter between critic and object an experience of "appreciation" and argues that without this kind of meeting, the resultant act will be one of processing data rather than one of critical engagement.[11] Black treats the conjunction between critic and object as the recreative aspect of criticism which makes "the critic a participant rather than a detached observer."[12] Evidently the recreative or appreciative phase of the critical act is the counterpart of reflexive symbolization in which observer and participant are briefly reunited. In rhetorical criticism, the pre-critical stage is a form of descriptive analysis.[13] A similar process is necessary for the criticism of communicative research—precisely what was done, what procedures were followed, what sampling techniques were used, what assumptions were made, what statistical tests were applied and why, and so forth.[14] The analogous process necessary for the criticism of theory is evident in the knowledge of details that underline Walter Carleton's response to an article by Thomas Farrell.[15]

[9] The concept of consciousness as simultaneously acting and aware of itself as consciousness is developed at length by Jean-Paul Sartre, *Being and Nothingness: An Essay On Phenomenological Ontology* trans. Hazel E. Barnes (New York: Philosophical Library, 1956). For specific applications to rhetoric, see Karlyn Kohrs Campbell, "The Rhetorical Implications of the Philosophy of Jean-Paul Sartre," unpublished Ph.D. dissertation, Univ. of Minnesota, 1968, 70-116.

[10] The pre-critical and critical stages of this encounter are discussed by Northrop Frye, *The Critical Path* (Bloomington: Indiana Univ. Press, 1971), 25-33; see also Campbell, 226-237.

[11] Lawrence W. Rosenfield, "The Experience of Criticism," *Quarterly Journal of Speech*, 60 (December 1974), 491.

[12] Black, *Rhetorical Criticism*, 43.

[13] Karlyn Kohrs Campbell, *Critiques of Contemporary Rhetoric* (Belmont Calif.: Wadsworth, 1972), 13-19.

[14] Leonard C. Hawes, "The Reflexivity of Communication Research," *Western Journal of Speech Communication*, 42 (Winter 1978), 12-20.

[15] Thomas B. Farrell, "Knowledge, Consensus, and Rhetorical Theory," *Quarterly Journal of Speech*, 62 (February 1976), 1-14;

As indicated earlier, this detailed examination shades imperceptibly into the critical act which comes into being when the critic apprehends the critical object as a unified whole. Descriptive analysis of the object(s) is a necessary but not a sufficient condition for a fully realized critical act. In all cases, the initial encounter must be unmediated by a predetermined perspective, ideology, or formula, a form of interference that renders the subsequent critical act fraudulent. The presence of the critical act is signified by the discovery or recognition of a structure that unifies all facets of the object. Such moments of apprehension are easily illustrated from rhetorical criticism. Black discovers the structure of a morality play in Chapman's Coatesville Address,[16] Windt discovers that the acts of Yippies and Cynics alike are diatribes,[17] Zyskind discovers that Lincoln's Gettysburg Address is preeminently deliberative rather than epideictic.[18] A parallel process is evident in criticisms of communicative research and theory. A recent essay by Jesse Delia is illustrative. His critique of research done on credibility of sources or *ethos* reveals the field-invariant assumptions underlying factor analysis, and he posits an alternative structure which would, he argues, account for these apparently conflicting findings.[19]

Because all human symbolic activity is reflexive, criticism is an inevitable part of symbolic action. This special relationship between symbolic action and its reflexive dimension is replicated in the special relationship between the critic and the object of criticism. Initially, the relationship is linear and analytical. As the pre-critical act becomes the critical act, the relationship between critic and object becomes synthetic and holistic, a perception of the object of criticism that comprehends its unifying structure.

The Cognitive Dimension

Whether used by rhetoricians or communicologists, the critical process makes conscious and explicit what may be nonconscious or even unconscious for the author(s) and what is unrecognized by a casual or non-expert audience. It is this phase of the critical process—the discovery of structure— which causes critics to be attacked as cold, distant, and intellectual. The critical act is a cognitive act; it is designed to make overt what has been hidden. This concept of the function of criticism informs the work of Claude Lévi-Strauss on the structure of myth,[20] and Michael McGuire's analysis of the structure of *Mein Kampf* illustrates its application in rhetorical criticism.[21]

In one sense, criticism may be defined as the discovery and articulation of the structures that inhere in symbolic acts. Criticism is cognitive in its efforts to make such structures explicit; conversely, this phase of critical activity is participatory, appreciative, and synthetic. In addition, it notes that the structures revealed are not invariably logical or discursive. The foundations of many theories are commitments to unprovable first principles; similarly, although the structures of rhetorical acts may be

Walter M. Carleton, "What is Rhetorical Knowledge: A Response to Farrell—and More," and Thomas B. Farrell, "Social Knowledge II, *ibid.*, 64 (October 1978), 313-334.

[16] Black, *Rhetorical Criticism*, 78-90.

[17] Theodore Otto Windt, Jr., "The Diatribe: Last Resort for Protest," *Quarterly Journal of Speech*, 58 (February 1972), 1-14.

[18] Harold Zyskind, "A Rhetorical Analysis of the Gettysburg Address," *Journal of General Education*, 4 (April 1950), 202-212.

[19] Jesse Delia, "A Constructivist Analysis . . ."

[20] Claude Lévi-Strauss, "The Structural Study of Myth," *The Structuralists from Marx to Lévi-Strauss* ed. Richard and Fernande DeGeorge (Garden City, N.Y.: Anchor, 1972), 169-174; originally published in *Journal of American Folklore*, 78 (Oct.-Dec. 1955), 428-44.

[21] Michael McGuire, "Mythic Rhetoric in *Main Kampf* . . ."

systems of coherent argument, they may also be archetypal metaphors, myths, or dramatic forms.

The reflexive basis for criticism and the unmediated pre-critical encounter which underlies critical action may seem to call into question the received wisdom that professional critics are better analysts of works than are their authors. The function of criticism as a revelation of structure explains why authors are less adept than professional critics. It is particularly difficult to recognize one's own assumptions or to unearth attitudes buried in one's choice of metaphor, for example. As Kenneth Burke so aptly noted, we all have an "occupational psychosis," a bias developed from the angle of vision which is habitually ours based on disciplinary area, occupation, or ideology.[22] It is as if we find it difficult to refocus our eyes to look through different lenses because we are accustomed to wearing a particular pair of spectacles. The dilemma regarding the conjunction of and the distance between the critic and the object of criticism seems most acute when the question is raised whether or not whites can criticize the rhetoric of blacks or men can criticize the rhetoric of feminists. Insofar as "white" refers to the perspective known as racism and "man" refers to the perspective known as sexism, they are, like all other perspectives adopted in advance, nearly insuperable obstacles to genuine critical activity.

The reflexive character of the critical act demands both a participatory encounter and the capacity to establish the aesthetic distance enabling one to perceive the object of criticism as a whole. In addition, the activity of criticism is rendered more complicated by the fact that rhetorical and communicative acts are, themselves, complex.

The Dialectical Dimension

The combative, conflicting, dialectical character of criticism in our discipline is a function of the qualities and characteristics of rhetorical and communicative acts, the fundamental units of study. In the terminology of Northrop Frye, criticism must be polymorphous (and perverse, too, in all probability) because communicative and rhetorical acts are polysemous.[23] His argument, as applied to literary criticism, may be summarized this way:

Literary acts are polysemous, that is, they "mean" in many different ways and on different levels. They mean in terms of theme (*dianoia*), narrative structure (*mythos*), and the phases of symbols; they mean as unique works, and they mean as members of classes or genres. As a result, literary criticism must take different forms and assume a variety of perspectives in order to account for the many dimensions of meaning that exist simultaneously in literary acts.

This argument appears to be equally forceful when applied to the analysis of communicative and rhetorical acts, a claim which is supported by the work of both rhetoricians and communicologists. Long-standing interest in the study of movements and more recent interest in the study of campaigns and genres reflect a recognition by rhetorical critics and students of persuasion that acts mean as members of groups in addition to the meanings they have as unique acts, the special concern of most Aristotelian analysis. The published essays of rhetorical critics demonstrate that rhetorical acts mean as mythic structures, as imaginative or dramatic works, as discursive arguments, and

[22] Kenneth Burke; *Permanence and Change: An Anatomy of Purpose* (1954; rpt. Indianapolis: Bobbs-Merrill, 1965), 37-49.

[23] Northrop Frye, *Anatomy of Criticism*, 71-73.

as cultural-psychological inducements, among others.

A similar picture is presented by the publications of communicologists. The analysis of invariant factors in communication is beginning to be balanced by studies and perspectives that reflect views of communicative processes as field-dependent.[24] The study of "talk" as "rule-governed" considers whether or not spontaneous acts of ordinary speech follow recurrent patterns. Systems analysis, discourse analysis, constructivism, ethnographic analysis, hermeneutic phenomenology, and the covering law perspective are names given to some of the differing theoretical views. Some of these perspectives in both rhetorical and communicative studies will be mutually exclusive; some will be more productive than others. However, the presence of many theoretical and methodological viewpoints is not a sign of chaotic instability but evidence of the health and maturity of our discipline which is becoming increasingly able to account for the many meanings of rhetorical and communicative acts.

A pluralistic view of criticism may appear to imply that all perspectives are equally valid. In one sense, that is true. Despite its "falsity," I live most of my life in conformity to the flat earth theory, and although they have been superceded, I lay out my garden in accordance with the principles of Euclidean geometry. Analogously, light is ever and always both a wave and a particle. Edwin Black's analysis of what he called "neo-Aristotelian" criticism has been read by some as a total condemnation of it. In fact, his analysis revealed that it was a system adapted to the recreation of the immediate historical occasion for examining highly discursive acts as individual works addressed to specific audience, and for the application of the effects criterion. Wayne Brockriede's recent reference to the excellence of Marie Hochmuth Nichol's essay on Lincoln's First Inaugural is a reminder of what the neo-Aristotelian critic does exceptionally well.[25] Similarly, more recent essays by Michael Leff and Gerald Mohrmann and by Forbes Hill[26] attest to the health of the Aristotelian tradition in criticism. What Black demonstrated so perceptively was the inadequacy of neo-Aristotelianism *as a monistic system of criticism*. Because it is a single perspective, it can see well only what is filtered through its particular lens. As Black illustrates, it is not a suitable critical perspective for understanding Chapman's Coatesville Address or for analyzing works which Black describes as belonging to the genre of exhortation. The same kind of argument can and should be made for any theory or methodology in rhetoric or communication which purports to be the single valid perspective.

A process analogous to that initiated by Black in rhetorical studies is now beginning in communication. A number of authors are now attacking "the hypothetico-deductive model" of empirical/quantitative research based on "logical positivism." Insofar as these essays seek to debunk a model for theory and research which claims to be the single valid approach to the study of communication, they are correct. Insofar as these authors propose equally monistic alternatives, they are open to the same

[24] See, particularly, the essays by Delia, Hawes, and Philipsen cited in note 6.

[25] Wayne Brockriede, review of *The Political Pulpit* by Roderick P. Hart, *Philosophy & Rhetoric*, 11 (Summer 1978), 203-4.

[26] Michael C. Leff and Gerald P. Mohrmann, "Lincoln at Cooper Union: A Rhetorical Analysis of the Text," and Forbes I. Hill, "Conventional Wisdom—Traditional Form: The President's Message of November 3, 1969," *Quarterly Journal of Speech*, 58 (December 1972), 373-386.

attacks, as Miller and Berger have perceptively noted.[27]

Because of the complexity of rhetorical and communicative acts, theoretical and methodological perspectives must vary in order to account for their many dimensions. In the case of rhetorical and communication, the existence of a single, dominant "paradigm" is evidence of disciplinary senility. Criticism of such a unitary modality is a sign of returning health, and competition among a variety of perspectives is indicative of maturity and vigor.

The Evaluative Dimension

The dialectical character of criticism means that evaluation will be an inevitable part of critical activity. At a minimum, the critical act unearths theoretical presuppositions, assumptions about the treatment of research data and underlying the choice of procedures, and conceptions of how rhetorical acts work. In all cases, critical analysis must assess the capacities and limitations of the perspective assumed by the rhetorical critic, the theorist, or the quantitative researcher. Even as it seeks to recognize and understand communicative and rhetorical acts, it is evaluative because it reveals the limitations of the choices made. In addition to this necessary quantum of evaluation, there are other critical judgments inherent in the critical act.

The most fundamental critical appraisal is artistic or aesthetic, a judgment about the quality or execution or performance. Such an appraisal follows from the primary purpose of criticism: increased understanding of the communicative and rhetorical acts of human beings. Critical description quickly becomes critical evaluation as critics distinguish between acts that are fully and partially realized. Rhetorical critics note elegance of argument and coherence of structure; critics of communicative studies discriminate between "clean" and "dirty" research; critics of theory contrast that which is developed and elaborated with that which is not.

These artistic judgments reflect the etymology of the term "critic" for a critic was one who applied "criteria" in order to sift or judge (critic, criterion, and criticism are all derived from the Greek verb *krinein* meaning to separate, discern, or choose).[28] The criteria applied by critics are models or touchstones. In some cases, these models are presumed to exist *a priori* in symbols, in natural law, or in the mind of god; in others, the models are presumed to develop inductively and dynamically. In all cases, criticism reflects the sense in which we are "rotten with perfection,"[29] seeking perfect acts and absolute norms. Because critics apply criteria, their mode of argument is forensic, and like lawyers in a courtroom, they may apply a norm vigorously, dispute which norm applies, re-interpret a norm, or use a given case to argue that a norm must be modified.[30] Because such argument is usually discursive, the evaluative dimension of criticism reinforces its character as a cognitive activity. Similarly, the forensic mode of argument is most appropriate for situations in which a pluralism of perspectives is presupposed.

If a criterion drawn from effects is taken to mean evaluation based on approval by or acceptability to a general

[27] Gerald R. Miller and Charles R. Berger, "On Keeping the Faith in Matters Scientific," 56.

[28] *The American Heritage Dictionary of the English Language* (Boston: American Heritage and Houghton Mifflin, 1969), 3i4, 1540.

[29] Kenneth Burke, "Definition of Man," *Language as Symbolic Action* (Berkeley: Univ. of California Press, 1966), 16-20.

[30] Lawrence W. Rosenfield, "Anatomy of Critical Discourse,' *Speech Monographs*, 25 (March 1968), 55-57.

audience, it is a criterion extrinsic to criticism. Such judgments render criticism fraudulent and deterministic because the criteria on which they are based are not drawn from the nature of the acts themselves. For example, whether or not a particular utterance produces "uptake" (is understood by the auditor) depends on such external factors as whether or not the auditor belongs to the same linguistic community as the speaker, normal hearing ability in the auditor, the noise level in the room, and so forth. That judgments based on such factors are external and unrelated to the character of the act itself is obvious. External criteria interfere with the kind of encounter that is essential to the critical act. If effect on a specific audience becomes the criterion, then external judgments drawn from other fields take pre-eminence, and the distinctive character of rhetorical and communicative acts is forgotten.

An appropriate judgment of effects is closely related to, if not identical with, the artistic judgment. It is, to use Burkean teams, an assessment of the formal charge coupled with the symbolic charge of a given work.[31] In other words, it is an assessment of the capacity of the work (speech, theory, research procedure or whatever) to produce effects in an audience based on its formal and substantive potency. As a critic, one asks how and why it is that a given theory has the capacity to account for data and to gain adherents, how and why it is that a methodology can validly and reliably generate and organize data, and how and why a speech has the ability to appear plausible, coherent, and moving. In short, it appraises the capacity of a symbolic act to induce participation and to organize material into a persuasive structure. This modification of the artistic criterion emphasizes the importance of the audience in all studies of rhetorical and communicative acts, but it does not make external criteria dominant considerations.

The criteria available to critics are ordinarily thought to be artistic, utilitarian or pragmatic, epistemic, and axiological. Criteria developed from considerations of "truth" and "ethics" can easily be external and deterministic. The problems of the truth criterion have been addressed in essays which consider what kind of epistemology is suitable for rhetoric and communication. These essays usually develop a position similar to the Aristotelian concept of *phronesis*, the Greek term for social truth, wisdom about political and social matters, or knowledge which is practical (related to *praxis*).[32] The authors seek to preserve the disciplinary autonomy which permits the unmediated encounter between the critic and the object of criticism. The issue is ever and always what epistemic standards are applicable to the study of communicative and rhetorical acts. Hawes' call for ethnographic studies to determine the nature of the fundamental unit of communicative studies[33] and Delia's recognition of the contingent character of communicative processes are critical examinations of a truth criterion suitable to this discipline.[34] The issue is difficult and remains unsettled, but appropriate questions are being framed: Are rhetorical and communicative truths situational? Are variables or factors in

[31] Burke, *Permanence and Change*, 24.

[32] In addition to the essays already cited by Farrell and Carleton, see Robert L. Scott "On Viewing Rhetoric as Epistemic," *Central States Speech Journal*, 18 (February 1967), 9-16, and "On Viewing Rhetoric as Epistemic: Ten Years Later," *ibid.*, 27 (Winter 1976), 258-266. Aristotle's classification of different kinds of knowledge is found in *The Nicomachean Ethics*, book 6.

[33] Leonard C. Hawes, "Toward a Hermeneutic Phenomenology of Communication," *Communication Quarterly*, 25 (Summer 1977), 30-41.

[34] Jesse Delia, "A Constructivist Analysis . . ."

communicative processes field-dependent? Must theories of communication and rhetoric be founded on presuppositions of meaning as constructed or aroused? All of these questions ask how the truth criterion may be legitimately applied in our field.

The application of the ethical criterion presents a parallel problem although less progress has been made toward developing appropriate axiological principles. In rhetorical studies, the norm has been the application of extrinsic criteria, and the exception has been the development of criteria drawn from the nature of rhetoric itself.[35] The most fundamental ethical analysis is an exploration of what can reasonably be expected to be the consequence, now and later, of rhetorical acts, of experiments and other research procedures, of theories, and of forms of criticism. The minimal ethical concern should be to protect the rhetorical and communicative acts we study. For this reason, all acts and procedures which deceive or coerce seem highly suspect. This reflexive view of the bases for ethics leads inevitably to the view that the kinds of critical activity I have described plays an essential role in the development and application of ethical norms. Criticism makes us conscious of what we are doing, how it is done, what it presumes, and of its capacities and limitations. It is, I think, an activity through which axiological principles for the discipline emerge.

Conclusion

It should now be evident that I believe that critical activity is the heart of our discipline. It is the energizing principle through which the discipline grows and develops; it is a sign of its maturity. The critical activities of many rhetoricians and communicologists have brought us to the current situation which, to me, contrasts favorably with conditions as they existed in 1965, a watershed year for rhetorical scholars. There is much more and better rhetorical criticism; there are critical essays from many more perspectives; there is a lively dialogue about the relative merits of different approaches to rhetorical criticism. Similarly, studies of communication have also made substantial gains. From a time when communicative research was almost wholly dependent upon theory and methodology drawn from outside our field, we have moved toward the studies and theories noted at the beginning of this essay which seek theory and research uniquely suited to the character of rhetorical and communicative acts. We have done much; there is still much to be done; but our critical activity should be a source of professional pride and optimism.

This essay claims that the characteristics of critical activity are a function of the nature of human symbolization. It seeks to demonstrate that criticism is ever and always reflexive, cognitive, dialectical, and evaluative. It attempts to show that these qualities of criticism explain and qualify the role of the critic's subjectivity in the act of criticism.

I have argued that criticism in rhetorical and communicative studies should be autonomous, independent of the theory, methods, and criteria of other disciplines and that its character and qualities can and should be drawn from the nature of the acts that it studies. This claim rests on an unproved assumption: that rhetorical and communicative acts constitute a coherent, symbolic order which can be mirrored

[35] An attempt to develop intrinsic criteria is made by Virgil Baker and Ralph T. Eubanks, "Toward an Axiology of Rhetoric," *Quarterly Journal of Speech*, 48 (April 1962), 157-168, and Ralph T. Eubanks, "Axiological Issues in Rhetorical Inquiry," *Southern Speech Communication Journal*, 1944 (Fall 1978), 11-24.

in the structure of knowledge in our discipline.

Many different disciplines are required to describe and understand all of the dimensions of human symbolic activity. Within our discipline, through the critical activity which continually refines our efforts and ensures a pluralism of perspectives for research and theory, we shall expand our ability to understand the various dimensions of rhetorical and communicative acts.

ON REDISCOVERING THE APPLE: SOME ISSUES IN EVALUATING THE SOCIAL SIGNIFICANCE OF COMMUNICATION RESEARCH

Gerald R. Miller

THIS paper rests on the assumption that one crucial role of criticism lies in assessing the social applicability—and hence, the social significance—of communication theory and research. Certainly, most students of communication pay lip service to this critical role; indeed, assertions such as, "Communication is a *social* process;" or "Communication is a vital aspect of humanity's *daily existence*" are commonplace. These assertions clearly imply that one critical yardstick for evaluating communication scholarship involves assessment of the extent to which it adds to our understanding of everyday communicative behavior. Yet despite many enthusiastic endorsements, criticism of the social significance of research is a relatively neglected activity. Instead, criticism typically focuses almost exclusively on theory and methodology, with the latter usually receiving more emphasis than the former. Seldom is discussion or analysis directed at the generalizability of research findings or the social utility of theoretical perspectives. As my comments will reveal, I think this is an unfortunate state of critical affairs.

Lest I be misunderstood, let me register two preliminary disclaimers. First, I do not mean to imply that the criticism of theory and methodology is unimportant; obviously, these are two scholarly staples of communication theory and research that merit careful evaluation. Second, and perhaps more important, I do not believe the three areas of theory, methodology, and social applicability are mutually exclusive, independent domains for criticism; indeed, certain methodological and theoretical questions are inextricably bound up in the assessment of social utility. What I am objecting to is the paucity of critical discourse that directly confronts the issue of *generalizability, social applicability,* or whatever label one wishes to attach to it.

Even if space permitted, I lack the cognitive wherewithal to identify all the potentially important critical issues that might be raised about the application of communication research to "real-world" situations. Consequently, I will be content to comment on two sets of issues that strike me as both germane and important: first, issues regarding the *logical assumptions of the traditional communication research model*; second, issues regarding the *ecological validity of typical communication research.*[1]

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[1] As best I can determine, the term "ecological validity" was coined by Egon Brunswik in his *Systematic and Representative Design of Psychological Experiments with Results in Physical and Social Perception* (Berkeley, Calif: University of California Press, 1947).

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Throughout the paper, I will rely on our research dealing with the use of videotaped testimony in courtroom trials for illustration.[2]

Issues Regarding the Logical Assumptions of Our Traditional Research Model

The model underlying most theory-building research in communication places a premium on rejection of the null hypothesis; in fact, it can be characterized as prejudiced against outcomes of no significant differences. Greenwald emphasizes the one-sided nature of the prevalent research model, noting that "The point is commonly made that theories predict relationships between variables; therefore, finding relationships between variables (i.e., non-null results) helps to confirm theories and thereby to advance science."[3]

Several writers have underscored the dangers of a model biased in favor of Type I error. For example, Bakan poses an intriguing hypothetical scenario culminating in a situation in which what appears to be a well-replicated finding of significant differences is actually an instance of Type I error.[4] Bakan reasons that since only a finite number of plausible hypotheses can be derived from extant social science theories, one hundred researchers could conceivably be conducting research on essentially the same hypothesis. Given the normative .05 level of significance, five of the one hundred studies would be expected to produce chance differences. Publication biases, stemming from allegiance to the logical assumptions of the prevailing research model, virtually assure that these five studies will be submitted and published, while the remaining ninety-five investigators will shelve their data and return sadly to their procedural drawing boards. As a consequence, what is trumpeted in research monographs and communication textbooks as a firmly established empirical generalization is actually an erroneous artifact of our scientific preoccupation with rejecting the null hypothesis.

Note that the pitfalls mentioned thus far pertain to communication research as a theory-building enterprise. Upon turning to the applied arena, the problem of Type I error bias is further compounded because of the numerous social, economic, and political considerations that often impinge on a research undertaking. As any debater knows, the need for a change in policy is always predicated on the assumption that certain advantages will accompany it. These advantages, in turn, must be weighed against possible disadvantages that may result from the change. When attempting to determine if such disadvantages are likely to occur, the communication researcher may face a situation in which acceptance of the null hypothesis is both likely and desirable. In other words, the researcher does not expect the new policy to yield significantly better outcomes than the old—though if it does, this is added icing on the policy maker's cake—but neither does s/he anticipate that it will prove significantly worse than the present alternative.

An example from our courtroom research will serve to illustrate this point. Proponents of videotape emphasize several advantages to be gained from its use in trial proceedings. Videotape can increase the efficiency with which trials

[2] The research alluded to was supported by NSF Grant #GI 38398, Gerald R. Miller and Fredrick S. Siebert, Principal Investigators; and by NSF Grant #APR75-15815, Gerald R. Miller, Principal Investigator.

[3] Anthony G. Greenwald, "Consequences of Prejudice Against the Null Hypothesis," *Psychological Bulletin*, 82 (1975), 2.

[4] David Bakan, *On Method: Toward a Reconstruction of Psychological Investigation* (San Francisco: Jossey-Bass, 1967), pp. 1-29.

are conducted and thereby help reduce backlog and clear crowded court dockets. Inadmissible testimony can be edited from the trial so that jurors are never exposed to its potential prejudicing effects. Geographically remote and professionally over-committed expert witnesses can give their testimony via videotape at their convenience. Finally, jurors' time can be spent more wisely, since prerecording of the entire trial on tape ensures that they will hear the case and deliberate on it, rather than being excused midway in the trial because the litigants have decided to settle.

On the other hand, some opponents of this judicial innovation contend it will have a negative impact on juror response; that is, jurors will be less interested in watching videotaped trials, will retain less of the important trial-related information, and will be hindered in their attempts to evaluate the demeanor of witnesses. In short, these individuals argue that the advantages to be realized from adopting videotape will be offset by a decline in effectiveness, a decline that underscores the wisdom of maintaining the present system of live trials.

Our own research has dealt primarily with the effects of videotaped court materials on juror response. More specifically, we have compared videotaped trials and depositions with their live counterparts in terms of their effects on such juror responses as verdicts and magnitude of awards, perceptions of the credibility of trial participants, retention of trial-related information, and self-reports of interest and motivation in the task of serving as jurors. In no instance have we observed any deleterious effects on juror response from using videotape, and in the case of information retention, our findings indicate that jurors recall more information when they view taped testimony.[5] Note, however, that the applied logic of this research situation does not require differences in favor of the videotape medium. Rather, given the other advantages to be gained from its adoption, a policy maker would be satisfied to know (or at least, to believe) that videotape is no worse than the live medium in its effects on the responses of interest.

How much confidence can be placed in findings of no difference? Speaking for myself, I am as confident of the results as of any I have observed in research in which significant differences were observed. As I shall emphasize later, the studies were conducted in environments closely approximating actual trial conditions. Instruments were carefully pretested and item analyzed. Jurors were compared on numerous demographic characteristics to ensure comparability of the various conditions. Power estimates were calculated to determine an adequate sample size for strong tests of the null hypothesis. In short, a concentrated effort was made in the studies to control for the various sources of Type II error.

Nevertheless, most communication researchers would probably feel uncomfortable about results which support the null hypothesis. As Greenwald notes, the

[5] See, for example, Gerald R. Miller, "Jurors' Responses to Videotaped Trial Materials: Some Recent Findings," *Personality and Social Psychology Bulletin*, 1 (1975), 561-569; Gerald R. Miller, "The Effects of Videotaped Trial Materials on Juror Response" in Gordon Bermant, Charlan Nemeth, and Neil Vidmar, eds., *Psychology and the Law: Research Frontiers* (Lexington, Mass.: D. C. Heath, 1976), pp. 185-208; Gerald R. Miller, David C. Bender, B. Thomas Florence, and Henry Nicholson, "Real versus Reel: What's the Verdict?" *Journal of Communication*, 24 (1974), 99-111; and Gerald R. Miller, David C. Bender, F. Joseph Boster, B. Thomas Florence, Norman E. Fontes, John E. Hocking, and Henry E. Nicholson, "The Effects of Videotaped Testimony in Jury Trials: Studies on Juror Decision-Making, Information Retention, and Emotional Arousal," *Brigham Young University Law Review*, 1 (1975), 331-373.

conventional wisdom holds that findings of no difference are an inferior scientific species, subject to hypercritical scrutiny.[6] Interestingly, one rarely observes a communication researcher engaged in serious soul-searching when s/he has found it possible to reject the null hypothesis, even though there is certainly room for healthy skepticism. After all, statistically significant findings only reveal that *something* produced a systematic association or difference, not necessarily that the variables of interest were responsible. If researchers are willing to applaud a colleague's sound craftsmanship for a study reporting significant differences, assuming, of course, that the research report does not contain clear evidence of methodological or procedural inadequacies, why are they not willing to render the same judgment for one supporting the null hypothesis?

What is needed, then, is a new attitude about the null hypothesis, not only in conducting research, but also in evaluating and criticizing it. Investigators should plan carefully to minimize sources of Type II error, not just to enhance the likelihood of rejecting the null hypothesis, as is presently the case, but also to bolster confidence in findings of no differences. Moreover, as Greenwald suggests, communication researchers should: "(1) Do research in which any outcome (including a null one) can be an acceptable and informative outcome; and (2) Judge their own (or others') research not on the basis of results but only on the basis of adequacy of procedures and importance of findings."[7] In short, researchers should avoid a myopic perspective on problems that lead them to construct research "loaded" in favor of rejecting the null hypothesis, particularly when the applied or theoretical logic of the situation implies that a finding of no differences would be useful and informative. Failure to adopt such a broad perspective could be cause for critical scrutiny.

Though I have argued that a changed attitude about the null outcomes would be especially useful in the applied arena, where such outcomes often provide valuable assistance in policy-making, I do not wish to short change the potential value of a new critical and methodological stance in the domain of traditional, theory-building research. *Communication science advances not only by confirming theories but also by rejecting them.* Indeed, most students of communication would accept the criterion of *falsifiability* as the minimum test of a useful theory. It is ironic that the field continues to champion the theoretical credo of falsifiability while at the same time clinging to methodological shibboleths which render falsification difficult, if not impossible. This theoretical/methodological schizophrenia is likely to be counterproductive for basic and applied communication research alike, yet research is seldom criticized for its schizophrenic tendencies.

Before turning to issues regarding the ecological validity of typical communication research, I want to examine briefly a kind of "twilight zone" problem, one that might conceivably be discussed in either of the two major sections of this essay. The problem concerns the kinds of measures, of dependent variables, that have preoccupied most communication researchers. With only occasional exceptions, communication researchers have relied on paper-and-pencil measures of underlying affective or cognitive states —*attitude, communicator credibility,* and the like. While these measures have much to commend them, they pose thorny problems when it comes to apply-

[6] Greenwald, 203.
[7] *Ibid.*, 19.

ing communication research to everyday social situations.

Since they are familiar to most readers, the arguments favoring such measures can be summarized succinctly. First, as any novice student of scaling knows, interval-level and ratio-level measurements produce more information, and most of the scales employed by communication researchers purport to yield these levels of measurement. Second, satisfactory explanation is typically conceived of in terms of relationships of concomitant variation (i.e., monotonic and nonmonotonic or linear and curvilinear functions), and interval- or ratio-level measurement is seen as comporting most closely this goal. Furthermore, explanation usually involves postulation of some mediating process (e.g., *attitude change* or *perceived credibility*), and although, in principle, there are no barriers to measuring these processes nominally or ordinally, tradition is solidly in the corner of interval and ratio scaling. Finally, in a more cynical vein, there appear to be at least two unstated reasons why communication researchers often cling to such measures; they are *convenient*, which facilitates the conduct of a great deal of "quick and dirty" research, and they have a *scientistic ring*, thereby permitting conferral of considerable self-respectability through endless squabbling and quibbling over esoteric, supposedly profound measurement issues.

Regardless of the relative merits of arguments favoring paper-and-pencil, interval- and ratio-level scales, the fact remains that their track record in predicting other kinds of behavior leaves much to be desired.[8] Moreover, these other kinds of behavior are likely to be of foremost concern in the applied arena. People's "real world" responses to messages are usually viewed in nominal or ordinal terms, not in so many units of some underlying affective or cognitive predisposition. Voters cast ballots for Carter or for Nixon; consumers purchase or do not purchase Crest toothpaste; love objects accept or reject the marital advances of ardent suitors; jurors find the plaintiff or the defendent. To be sure, determined supporters of traditional measurement tools might contend that these discrete actions are best explained by recourse to some complex antecedent structure of values and attitudes. But in everyday communicative transactions, the proof of the pudding lies in eliciting the desired behavior, and this argument might gain a more sympathetic hearing if it could be convincingly demonstrated that knowledge about such structures permits accurate prediction of other responses.

To carry this point a step further, it is instructive to note that most research areas of admitted applied impact are grounded in nominal or ordinal measurement. Consider two examples, public opinion polling and operant conditioning. Although pollsters have occasionally tinkered with interval scales, the "bread-and-butter" measures remain nominal and ordinal. Respondents are asked to indicate which candidate will receive their vote, including the option of responding "Undecided." These choices are, in turn, correlated with demographic measures which are themselves nominally and ordinally scaled—sex, race, religious preference, socioeconomic status, etc. This modest approach to measurement has permitted the electorate to know the outcomes of major elections on the basis of projections from a small percentage of tabulated votes, a predictive batting average that any

[8] For a useful discussion of this problem, see the papers in Allen E. Liska, ed., *The Consistency Controversy: Readings on the Impact of Attitudes on Behavior* (New York: Wiley & Sons, 1975).

persuasion researcher should envy and covet. Similarly, operant conditioning, which has enjoyed considerable success in educational and therapeutic settings, usually relies on discrete, dichtomous behaviors as the basic unit of measurement. Granted, operant conditioning procedures permit the generation of all forms of learning curves, but the raw data for these curves consist of frequency counts of discrete behaviors—pressing or not pressing a bar, saying "Thank you" or not saying "Thank you," etc. Indeed, any scheme to construct an interval- or ratio-level scale to measure children's attitudes toward observing proper social amenities would surely be greeted with mirth or derision by B. F. Skinner, the father of the operant conditioning approach.

In short, the message is as follows: if if communication researchers wish to reap applied harvests from their labors, they may need to abandon their slavish commitment to a measurement model grounded in interval and ratio scaling and commence the task of examining other behavioral measures of communicative effects. Certainly, such a reorientation does not require total renouncement of traditional scaling assumptions; indeed, such recent work as that of Barnett, Serota, and Taylor provides persuasive evidence of the applied utility of certain ratio-level scaling procedures.[9] Still, there is no reason to rejoice about the overall predictive record of our typical measures in the realm of everyday communicative transactions, nor is it likely that further nitpicking refinement of existing scales or the addition of yet another intervening variable to present cognitive equations will rectify the problem. Rather, researchers need to manifest greater concern for the kinds of responses communicators seek to elicit in their daily transactions. Furthermore, critics of communication research should not hesitate to raise questions about the social significance of research that departs radically from this response domain.

[9] George A. Barnett, Kim B. Serota, and James A. Taylor, "Campaign Communication and Attitude Change: A Multidimensional Analysis," *Human Communication Research*, 2 (1976), 227-244.

Issues Regarding the Ecological Validity of Typical Communication Research

Anyone who has ever described a communication study to a group of intelligent laypersons, unschooled in the procedural and methodological mysteries of social science research, knows that the first flurry of questions from these novice critics will inevitably concern the generalizability of the research findings. Will these findings hold for a different group of participants? Will they hold if the data are collected in a different research environment? Will they hold if the variables are operationalized differently? The list of these questions is familiar to most communication researchers.

A set of learned-sounding answers has been developed to parry these critical queries, some of which are calculated to carry the day on jargon alone. Invoking such concepts as *randomization, matching, binomial probability distribution,* and *analysis of covariance*—to mention but a few—usually ensures resigned silence, if not informed understanding of the limits of the research. Still, it is undoubtedly a rare communication researcher who has not suffered from moments of uncertainty and skepticism about the ecological validity of his or her research findings. Furthermore, five years of applied research in the legal sector have convinced me that this uncertainty and skepticism are warranted

and healthy, for as Kelman reminds us, "the gap between the laboratory and the real world is so great that one is hardly justified in the conclusion that what has been established in the laboratory constitutes a verified fact about nature."[10] Indeed, issues regarding ecological validity erect the most troublesome barriers to confident application of communication research in "real world" settings. To illustrate why, let us examine two related sets of issues concerning generalizability: issues relating to the participants used in the research and issues relating to the environment in which the research is conducted.

Issues Relating to Research Participants

The venerable "college sophomore" remains the staple human commodity for the vast majority of communication studies, a fact attested to by the numerous quips and sallies regarding the perennial hardiness of this participant species. The continuing popularity of the species, *akademia moros,* is not difficult to understand. They are a plentiful species, readily available for research participation. Moreover, they are usually cooperative and/or compliant, particularly since they understand the researcher's frequent ability to mete out rewards and punishments. Small wonder, then, that the empirical justification for most current communication theory is rooted in this species.

There are, however, two good reasons for exerting caution when generalizing results obtained with college undergraduates to other populations. First, this group has a number of personal and demographic characteristics which distinguish its members from other participant populations. College students represent a restricted age range, and they are more intelligent than the majority of potential research participants. Thus, when we have examined the demographic characteristics of actual impaneled jurors, they have seldom "fit" either the age span or the cognitive abilities of a college undergraduate population. Typically, actual jurors are older and less educated, suggesting that they also differ in political and social attitudes and in socioeconomic background. Though we have used role-playing student jurors in our studies infrequently, our limited experience reinforces the belief that they do not provide a sound basis for generalizing to the actual trial setting. In one study dealing with retention of trial-related information, the mean retention scores of role-playing student jurors were practically at ceiling in all treatment conditions, preventing any test of between-condition differences.[11] By contrast, retention scores of actual jurors were much lower, permitting us to carry out the appropriate tests for the various conditions. Furthermore, several collateral studies conducted with college participants reveal a marked liberal bias in verdicts; i.e., college "jurors" are more likely to find for the defendent than are members of actual juries. Consequently, until such time as college students are substantially represented in the jury system, generalization of results from classroom to courtroom remains highly suspect.

The second reason for exercising caution in generalizing is even more troublesome: because they are often used as participants, college undergraduates are a research-wise species. Their wisdom not only makes them more adept at fathoming the *demand characteristics* of

[10] Herbert C. Kelman, *A Time to Speak: On Human Values and Social Research* (San Francisco: Jossey-Bass, 1968), p. 158.

[11] Miller, *et al.*, 1975.

a study,[12] it also increases the likelihood that they will arrive at the study in a suspicious frame of mind. Since some form of deception is common practice in communication research, it is only natural that battle-scarred participants will attempt to "sniff out" the latest subterfuge, even if none exists. Elsewhere, I have described some of the research consequences of participant suspicion in the following way:

> subjects enter the laboratory with expectations about the researcher's purposes, and, in turn . . ., these expectations affect the subjects' behavior. If one of the shared prior assumptions of subjects is that experimental tomfoolery is inevitable, it becomes even more doubtful that they will perceive the situation in the way the researcher had hoped. Kelman (1967) has underscored this methodological dilemma forcefully, stating that "deception increases the subject's tendency to operate in terms of his private definition of the situation, differing (in random or systematic fashion) from the definition the experimenter is trying to impose; moreover, it makes it more difficult to evaluate or minimize the effects of this tendency" (7).
>
> [In addition] excessive deception imposes severe theoretical limitations on the communication researcher. Obviously, . . . the goal of scientific research in communication is the development of a set of comprehensive generalizations that will enable us to explain and predict human communication behavior. If these generalizations are tested with subjects drawn entirely from a population of suspicious persons, the domain of explanation is limited to that population; *in a sense, we have developed a theory of communication that deals only with suspicious persons.* Since much daily communication occurs within a psychological atmosphere of trust and good will, our theory is irrelevant to numerous interesting and important communication phenomena.[13]

Although the preceding remarks focus on theory-building research, they are equally germane to the applied goal of generalizing research findings to ongoing social settings. Unless there are grounds to believe that the setting itself is characterized by mistrust, results obtained with suspicious research participants are of dubious ecological validity.

Again, an example from our courtroom research illustrates my argument nicely. During a study conducted with actual jurors in the Genessee County Court (Flint, Michigan), it was desirable for the jurors to believe the reenacted trial they were watching was an actual case in which their verdicts would be binding on the litigants. At the same time, financial and research constraints demanded some slight departures from normal trial procedures. Before the trial began, the presiding judge explained and justified the departures from normal procedure to the jurors. When questioned after the trial, not a single juror expressed the slightest suspicion about its validity, nor did any of them manifest behaviors during the trial that could be taken as indicators of doubt or suspicion. Instead, all jurors stated that they believed they were involved in an actual trial and that their verdicts would be binding.

In sharp contrast, we have yet to execute a study using role-playing student jurors without encountering numerous questions about "the real purpose of the study." Even when no deception is involved, student "jurors" seem bound and determined to discover some; their past experiences with social science research lead them to believe that subterfuge is an indispensable ally of the communication researcher. Although they can hardly be blamed for their suspicions, it is patently clear that their expectations vary radically from

[12] For a discussion of the concept of demand characteristics, see Martin Orne, "On the Social Psychology of the Psychological Experiment: With Particular Reference to Demand Characteristics and Their Implications," *American Psychologist*, 17 (1962), 776-783.

[13] Gerald R. Miller, "Research Setting: Laboratory Studies" in Phillip Emmert and William D. Brooks, eds., *Methods of Research in Communication* (Boston: Houghton Mufflin, 1970) pp. 101-102.

those of actual jurors in a courtroom situation.

By now, the general thrust of these remarks should be apparent. If communication researchers wish to place much confidence in the ecological validity of their findings, greater care must be exercised in selecting research participants whose characteristics comport closely with the parent population(s) of interest. The fact that student participants are handy and tractable does not justify their inclusion in studies, particularly if the researcher is concerned with the practical application of his or her results. Granted, efforts to obtain more representative samples are almost certain to exact additional costs in both time and money. Still, if the goal is theoretically relevant and socially significant research findings, the game is surely worth the candle. At the very least, communication researchers should stop deluding themselves with the wishful thought that exclusive reliance on the species, *akademia moros,* will yield empirical generalizations which meet the predictive tests of the communicative marketplace. In seeking to shed this delusion, they might well be aided by cogent criticism focused on the potential shortcomings of various participant populations.

Issues Relating to Research Environments

At the outset of our research dealing with the effects of videotaped materials in courtroom trials, many lawyers and jurists emphatically stated they would not place an ounce of confidence in results obtained using simplified, truncated simulations of the courtroom environment. As one for whom such simulations were a daily fact of research life, I initially reacted negatively to their disclaimers. Five years later I am convinced: the typical social scientific attempt to study courtroom communicative processes and the behavioral milieu of the courtroom are miles apart; indeed, one could even say they constitute two very different worlds.

Consider this common procedural scenario for courtroom communication studies. College students are assembled in a campus classroom or laboratory and told they will be assuming the role of a trial juror, a task that confers little social, ethical, or legal responsibility on them. They are then handed a one or two page, written summary of trial arguments or testimony and asked to read it. Usually, some variable is systematically manipulated in the summary; e.g., an item of inadmissable testimony may be included in one written account but not in the other. After the "jurors" have read this abbreviated account of the trial, they are asked to render a verdict, fix severity of sentence, or provide other similar dependent measures. The students then depart to the library or the local watering spa, while the researcher heads back to his or her office to begin coding the data.

Now imagine the atmosphere in an actual courtroom. Jurors embark on their task with a sobering sense of responsibility, for their decisions often entail substantial personal and/or economic consequencies for the litigants. They are faced with a rich stimulus field consisting largely of a series of complicated communicative exchanges. Rules are invoked; objections are lodged; perceptions of credibility wax and wane as the dialogue continues. The verbal and nonverbal demeanor of witnesses and attorneys must be evaluated; the quality of arguments and evidence must be weighed; the instructions of the judge must be comprehended. If the case has attracted considerable pretrial publicity (or in many cases, even if it hasn't), the

jurors must strive to rise above their prejudices and to render the most objective decision possible. In short, the actual trial is an incredibly complex psychological, sociological, and communicative event.

To equate this event with the simple research simulation described earlier is an act of scientific folly. The number of relevant variables that may shape the perceptions and decisions of actual jurors far outweigh those present in the campus research setting. At best, the latter's truncated simplicity unduly heightens the salience of the manipulated variable or variables; at worst, it renders the demand characteristics of the situation transparent. Although the findings of such a research undertaking may have some theoretical import, their ecological validity is open to serious question.

Our legal research again provides a convenient vehicle for illustrating the thrust of my argument. In a study using the kind of truncated simulation procedures mentioned above, Sue, Smith, and Caldwell (1973) found that a single item of inadmissible evidence, unfavorable to the defendant, was sufficient to increase significantly the number of guilty verdicts returned by role-playing jurors.[14] During our own research program, we have conducted several studies in which the number of instances of inadmissible testimony occurring in a three and one-half hour civil trial was systematically manipulated.[15] In no instance has the presence or absence of inadmissible testimony exerted a significant impact on the verdicts or awards returned by trial jurors. Stated differently, the jurors are apparently capable of following the judge's instructions to disregard the inadmissible material in reaching their verdict. This conclusion is buttressed by observations of the jurors during group deliberation. In only one instance was inadmissible testimony introduced into the deliberation, and in this case the other jury members quickly reminded the juror who introduced it that the information was not to be considered.

To understand the discrepancy between the findings of Sue, *et al.* and our own research, one need only contemplate the probable psychological impact of a single item of inadmissible testimony contained in a one page, written summary of a trial as contrasted with six items of inadmissible testimony interspersed in three and one-half hours of continuous argument and testimony. In the latter situation, the overall contribution of the six items of inadmissible testimony to the total trial process is relatively small; in the former instance, the single item of inadmissible material assumes considerable salience to the role-playing juror. Because of differences such as these, attempts to generalize from simple research simulations to actual trials are fraught with danger.

Similar problems arise when attempting to assess the ecological validity of several studies dealing with the influence of a defendant's physical attractiveness on subsequent juridic judgments.[16] Using truncated simulation procedures in which role-playing jurors were shown

[14] Stanley Sue, Ronald E. Smith, and Cathy Caldwell, "Effects of Inadmissible Evidence on the Decisions of Simulated Jurors: A Moral Dilemma," *Journal of Applied Social Psychology*, 3 (1973), 345-353.

[15] Norman E. Fontas, Gerald R. Miller, and David C. Bender, "Deletion of Inadmissible Materials from Courtroom Trials: Merit or Myth?" *Detroit College of Law Review*, 1 (1977), 67-81.

[16] See, for example, Michael G. Efran, "The Effect of Physical Appearance on the Judgment of Guilt, Interpersonal Attraction, and Severity of Recommended Punishment in a Simulated Jury Task," *Journal of Research in Personality*, 8 (1974), 45-54; and Harold Sigall and Nancy Ostrove, "Beautiful but Dangerous: Effects of Offender Attractiveness and Nature of the Crime on Juridic Judgment," *Journal of Personality and Social Psychology*, 31 (1975), 410-414.

pictures of defendants before reading brief descriptions of the trial, the researchers report systematic relationships between the defendant's physical attractiveness and such juror judgments as verdict and severity of sentence. In an effort to replicate these findings in a more realistic setting, Fontes, Sunnafrank, and Miller prepared four videotapes of a one hour trial.[17] The only variable elements of the trial were the sex and the relative physical attractiveness of the defendant: the four conditions used were female/attractive; female/unattractive; male/attractive; and male/unattractive. No relationships were observed between the sex or attractiveness of the defendant and the subsequent severity of juridic judgments. Moreover, initial judgments of attraction were apparently modified by the communicative transactions that occurred during the trial. For instance, if a defendant were initially judged as highly attractive, but if s/he presented testimony in a hestitant, unsure manner, post-trial ratings of perceived attractiveness were significantly lower. Conversely, if initially unattractive defendants testified confidently, post-trial ratings revealed that their perceived attractiveness increased significantly. The moral for communication researchers is straightforward and cogent: simplification of a complex communication event may eliminate relevant variables that exert a strong impact in real world settings. Although such truncation may be justified theoretically, it does not bode well for generalization of the findings.

As a result of these kinds of discrepancies, the following procedural rule-of-thumb regarding the research environment seems warranted: *the greater the verisimilitude between the research environment and the real world setting being simulated, the higher the likelihood that research will be ecologically valid.* If researchers wish to argue convincingly for the applied significance of their findings, they cannot content themselves with research settings that are, at best, pale approximations of the natural environments they are seeking to understand. Obviously, added costs are associated with maximizing realism; the major reason we have been able to conduct research in actual courtroom settings is because our funding has provided us with the economic means to do it. Still, researchers can probably heighten considerably the realism of many research environments without dramatic increases in economic costs. More extensive use of field experiments provides but one means of simulating real world settings more closely. One thing is certain; if communication researchers do not wage a concerted campaign to rid themselves of the cloistered confines of the laboratory, they should not be surprised if others continue to question the ecological validity of their research undertakings. Informed criticism of the potential social significance of communication research could ensure the continued salience of issues regarding ecological validity. Hopefully such criticism will emerge in our journals with greater frequency.

[17] Norman E. Fontes, Michael Sunnafrank, and Gerald R. Miller, "Effects of Defendents' Physical Attraction on Juridic Judgments," unpublished manuscript, Department of Communication, Michigan State University, 1977.

CRITICISM OF COMMUNICATION THEORY AND RESEARCH: A CRITICAL CELEBRATION

David R. Seibold

Introduction

Criticism has enjoyed a vital role in rhetorical studies, where theory and criticism have assumed a reciprocal relationship.[1] Rhetorical theorists and critics have been wedded for better, not worse, despite strife over such central issues as which objects are appropriate for critical appraisal,[2] the methods of rhetorical criticism,[3] the critic's role in critical acts,[4] and the purposes served by criti-

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[1] For treatments of the relationship between rhetorical theory and rhetorical criticism, see Edwin Black, *Rhetorical Criticism: A Study in Method* (New York: Macmillan, 1965); Lawrence W. Rosenfield, "The Anatomy of Critical Discourse," *Speech Monographs*, 25 (1968), 50-59; and Robert L. Scott, "On *Not* Defining Rhetoric," *Philosophy & Rhetoric*, 6 (1973), 81-96.

[2] For discussion justifying, jabbing at, or adjudicating whether social movements, nonverbal behaviors, worldviews, social clashes, messages, paramessages, current events, and a myriad of other communicative artifacts fall within purview of the rhetorical critic, see Anthony Hillbruner, "Creativity and Contemporary Criticism," *Western Speech*, 24 (1960), 5-12; Theodore Otto Windt, Jr., "The Diatribe: Last Resort for Protest," *Quarterly Journal of Speech*, 58 (1972), 1-14; Paul I. Rosenthal, "The Concept of the Paramessage in Persuasive Communication," *Quarterly Journal of Speech*, 58 (1972), 15-30; Karlyn Kohrs Campbell, "Carroll C. Arnold: Two Views of Criticism," *Today's Speech*, 22 (1974), 39-41; Kathleen Jamieson, "The Rhetorical Manifestations of Weltanschauung," *Central States Speech Journal*, 27 (1976), 4-14; James W. Chesebro and Caroline D. Hamsher, "Rhetorical Criticism: A Message-Centered Procedure," *Speech Teacher*, 22 (1973), 282-290; Leland M. Griffin, "The Rhetorical Structure of the 'New Left' Movement: Part I," *Quarterly Journal of Speech*, 50 (1964), 113-135; Herbert W. Simons, "Requirements, Problems, and Strategies: A Theory of Persuasion for Social Movements," *Quarterly Journal of Speech*, 56 (1970), 1-11; Robert S. Cathcart, "New Approaches to the Study of Movements: Defining Movements Rhetorically," *Western Speech*, 36 (1972), 82-88; and David Seibold, "Jewish Defense League: The Rhetoric of Resistance," *Today's Speech*, 21 (1973), 39-48. Recently there has even been debate on the appropriateness of murder and suicide as critical objects: see Jeanne Y. Fisher, "A Burkean Analysis of the Rhetorical Dimensions of a Multiple Murder and Sucide," *Quarterly Journal of Speech*, 60 (1974), 175-189, and, in the same issue, Parke C. Burgess, "Murder Will Out-But As Rhetoric?" pp. 225-231 followed by Fisher's reply. "Rhetoric as More than Just a Well Man Speaking: A Rejoinder," pp. 231-234.

[3] Discussion in this area has ranged from calls for stronger critical methodologies [Marie Hochmuth Nichols, *Rhetoric and Criticism* (Baton Rouge: Louisiana State University Press, 1963)]; to attack on traditional methods (Black, *Rhetorical Criticism*); to response "to the current confusion in criticism" resultant from tendencies to "oversimplify critical form so as to include all methods or to make criticism so individual as virtually to exclude any method" [Robert L. Scott and Bernard L. Brock, *Methods of Rhetorical Criticism: A Twentieth Century Perspective* (New York: Harper & Row, 1972), vii]; to criticism of some rhetorical criticism for not being rhetorical [Donald C. Bryant, *Rhetorical Dimensions in Criticism* (Baton Rouge: Louisiana State University Press, 1973)].

[4] Divisiveness here has centered around reaction against the traditional critic as a disinterested, non-participant evaluator, and preference for critics "as participants and active, shaping agents (persuaders) affecting communication processes within their cultures" (see James W. Chesebro and Caroline D. Hamsher, "Contemporary Rhetorical Theory and Criti-

cism.[5] Even when they have chosen each other as the objects of criticism, observers cannot help but admire the vigor of the arguments and the vitality these differences signal for this province. For rhetorical scholars have recognized the disciplinary rigor and potential knowledge gain resultant from systematic description, analysis, and evaluation of rhetorical acts, rhetorical theories, and rhetorical criticism itself through epistemological, metaphysical, and axiological filters. If anything negative be said, perhaps it is that rhetoricians have worried too much about their methods, criticism, and status, failing Inge's reminder that "Worry is interest paid on trouble before it falls due."

Criticism has been quite another matter in communication theory and research.[6] Within the positivistic confines of variable-by-variable stockpiling of findings, too little effort has been devoted to critical assessment of these findings, of their finders, or of the foundling communication science. Additionally, efforts by non-scientific writers have been ignored or not taken seriously.[7] Aside from early debate over the focus of communication,[8] communication scientists have

cism: Dimensions of the New Rhetoric," *Speech Monographs*, 42 (1975), p. 312); calls for recognition of the moral role of the critic—"he must seek the truth, tell the truth, and expose lies" —in understanding rhetorical criticism (see Anthony Hillbruner, "The Moral Imperative of Criticism," *Southern Speech Communication Journal*, 10 (1975), p. 228); and argument that criticism is "an interpretive activity involving application of specialized representational schemas to rhetorical experiences," and no critic is representation or interpretation free (see David L. Swanson, "A Reflective View of the Epistemology of Critical Inquiry," *Communication Monographs*, 44 (1977), p. 219).

[5] While not necessarily incompatible, the following emphases reflect the variance in rhetorical scholars' construals of the functions of criticism: criticism as "pre-scientific" [John Waite Bowers, "The Pre-Scientific Function of Rhetorical Criticism" in Thomas R. Nilsen (Ed.), *Essays on Rhetorical Criticism* (New York: Random House, 1968), pp. 126-145] and criticism as a precursor to rhetorical theory-building (see Roderick P. Hart, "Theory-Building and Rhetorical Criticism: An Informal Statement of Opinion," *Central States Speech Journal*, 27 (1976), 70-77); criticism as, fundamentally, a sharing of one's view of social reality with other human beings (Philip Wander and Steven Jenkins, "Rhetoric, Society, and the Critical Response," *Quarterly Journal of Speech*, 58 (1972), 441-450; criticism as a basically humanistic endeavor [Carroll C. Arnold, *Criticism of Oral Rhetoric* (Columbus, Ohio: Charles E. Merrill, 1974)]; criticism as informed by rhetorical theory for "variety in view and freshness in approach" (see Otis M. Walter, "On the Varieties of Rhetorical Criticism," in Nilsen, *Essays on Rhetorical Criticism*, p. 168); criticism as a medium to "clarify and define the theoretical basis of public address" [Lester Thonssen, A. Craig Baird, and Waldo W. Braden, *Speech Criticism*, 2nd Ed. (New York: Ronald Press, 1970), p. 23].

[6] Robert L. Scott describes the relationship between "rhetoric" and "communication" this way: "rhetoric is communication characterized by a high degree of intentionality and a high degree of strucure, including distinctness of communicative roles; it eventuates in discourse in the public realm of experience rather than the private. . . . in the late twentieth century it is important to picture rhetoric as one of several sorts of communication." See "A Synoptic View of Systems of Western Rhetoric," *Quarterly Journal of Speech*, 61 (1975), 440. Scott elaborates on the limits of communication, especially "intentionality" as a distinguishing feature of communication as a "social" system, in "Communication as an Intentional, Social System," *Human Communication Research*, 3 (1977), 258-268. Scott's distinction in no way obviates the "one-worldness" of rhetoric and communication, and I am in agreement with Carroll C. Arnold on this point: see "Rhetorical and Communication Studies: Two Worlds or One?" *Western Speech*, 36 (1972), 75-81.

[7] See, for example, Stanley Deetz, "An Understanding of Science and a Hermeneutic Science of Understanding," *Journal of Communication*, 23 (1973), 139-157; Kenneth R. Williams, "Reflections on a Human Science of Communication," *Journal of Communication*, 23 (1973), 239-250; Raymond Williams "Communications as Cultural Science," *Journal of Communication*, 24:3 (1974), 17-25; Paul Newell Campbell, "The *Personae* of Scientific Discourse," *Quarerly Journal of Speech*, 61 (1975), 391-405; Daniel J. O'Keefe, "Logical Empiricism and the Study of Human Communication," *Speech Monographs*, 42 (1975), 169-183; Philip C. Wander, "The Rhetoric of Science," *Western Speech*, 40 (1976), 226-235.

[8] In particular, see the running commentary in the *Journal of Communication* (*JoC*): Dean C. Barnland, "Toward a Meaning-Centered Philosophy of Communication," *JoC*, 12 (1962), 197-211; Gerald R. Miller, "On Defining Communication: Another Stab," *JoC*, 16 (1966), 88-98; George Gerbner, "On Defining Communication: Still Another View," *JoC*, 16 (1966), 99-103; Frank E. K. Dance, "The Concept of Communication," *JoC*, 20 (1970), 201-210.

not often moved beyond theory or data to ask from whence they came and where they lead. It is as if communication scientists have taken seriously Benjamin Franklin's dictum, "Better a little with contentment than a lot with contention." And, when undertaken, criticism has been informed by curious motives and interpretations of the critical role: criticism of others borne of little personal inventiveness (recall Dryden's castigation of "They who write ill, and they who ne'er durst write, turn critics out of revenge and spite"); or criticism as a medium for wielding personal power (a tendency reminiscent of Samuel Johnson's snippet that "Criticism is a study by which men grow old and formidable at very small expense"). Criticism in communication science has proved more the art of condemnation than discrimination, and those who have witnessed the annual bloodletting by critics on our conventions' quantitative programs may agree that today's critic typifies the secondary definition of critic offered in Dr. Johnson's 18th century dictionary: "A censurer; a man apt to find fault." Lest I be accused of the same predilection, let me move to the following observations which are intended to provide the foundation for this essay.

In preparing this essay, one area I examined was the sociology of science including relevant papers in recent issues of *American Sociologist, American Sociological Review, Sociometry,* and *American Journal of Sociology.* I was struck by the many "critical" articles published in those journals (e.g., critiques of that field, specific theories, or individuals' interpretations of theories and data). With the assistance of Rick Thomas of the University of Illinois, I examined the articles published in those four American Sociological Association journals during the five years of 1972 to 1976 inclusive. We looked at nearly 16,000 printed pages and found that over 2,500 pages involved critiques of the types mentioned above. We then compared this rate of criticism (16%) with the percent of critical pages printed in comparable journals and professional magazines in our field during the same period. Specifically, we looked at over 8,000 pages of articles published in *Speech Monographs, Quarterly Journal of Speech, Journal of Communication,* and *Spectra.* Only 6.5% of those pages contained critical articles, forum entries, colloquy, or rejoinders. In short 6.5% of the pages in our publications reflected attempts at critical evaluation of speech communication, whereas twice and half again that percentage of the space in sociology publications was devoted to critical appraisal of that discipline.

I will have more to say about that survey. For now I wish to emphasize that this relative paucity of critical evaluation of communication's epistemological foundations, current theories, empirical methods, and research findings belies one of the hallmarks of any science; its aporetic, questioning, skeptical posture. Criticism is a canon of science and, if communication is to assume a favored position among the social sciences,[9] constructive evaluation must be encouraged in "communication science."[10] Recently Dr. Gerald Miller

[9] A telling indication that communication science has not yet achieved this status in the social sciences is the fact that the National Science Foundation (NSF) has refused to recognize communication as a primary discipline, despite a specific request for such status by the SCA Research Board. Communication appears only as an "interdisciplinary" field under one of the NSF divisions (Research Applied to National Needs, RANN).

[10] I use the term "communication science" to refer to that social system of scholars, whether within our discipline or not, who view themselves philosophically and ethically bound by the norms of science (described later); who seek, by means of systematic, replicable methods, the discovery of publicly certifiable and reproducible knowledge of communication-related

prescribed a "massive dose of Vitamin-C" to cure the ills of the speech communication field, including credibility, clout, coherence, continuity, confidence, and commitment.[11] The subfield of communication science has also suffered for want of another in the Vitamin-C line: *criticism*, or systematic scrutiny of the values, presuppositions, methods, knowledge claims, and applications of communication theory and research. The purpose of this essay is to celebrate and certify the rightful role of criticism in communication science by (1) examining the norms of science and suggesting the functions of criticism in "ideal" science; (2) contrasting the ideal with the *status quo* in our discipline; (3) suggesting appropriate changes in the structures of communication science so as to promote critical evaluation and facilitate critical expression.

Normative Basis for Criticism in Science

Science, writes Robert K. Merton,

> is a deceptively inclusive word which refers to a variety of distinct though interrelated items. It is commonly used to denote (1) a set of characteristic methods by means of which knowledge is certified; (2) a stock of accumulated knowledge stemming from the application of these methods; (3) a set of cultural values and mores governing the activities termed scientific; or (4) any combination of the foregoing.[12]

While most observers of science, from philosophers to science historians, have concerned themselves with the first three of these matters, it is only recently that attention has been directed to their combination as a systemic enterprise. Norman Storer, in particular, has written of the "social system of science," that complex pattern of interactions within a distinguishable community of scientists which is governed by shared norms and influenced by other social institutions in the environment.[13] Rather than viewing science as an "institution" whose goal is the generation of knowledge for application by other social institutions, the science-as-system approach looks inward to science "as a particular sort of social behavior, one which, for all its important relations with the rest of society, can be studied as an independent part of society rather than merely as part of a larger one."[14] This focus on the internal organization of science is relatively recent, following primarily from Merton's papers in the 1930's and 1940's on the structures of science which functionally facilitate its roles and values within the larger social order.[15] Thereafter, the works of Barber,[16] Shils,[17]

phenomena for purposes of prediction and control of those phenomena; and who recognize and legitimate each others' claims of membership in the scientific community.

[11] Gerald R. Miller, Untitled Paper Presented at Program on "The Goals of the Association: Three Views," Annual Convention of the Speech Communication Association, San Francisco, December, 1976.

[12] Robert K. Merton, "Science and Democratic Social Structure," in Robert K. Merton, *Social Theory and Social Structure*, 1968 Enlarged Edition (New York: The Free Press, 1968), p. 605. All subsequent references to this source are to the 1968 edition.

[13] Norman W. Storer, *The Social System of Science* (New York: Holt, Rinehart and Winston, 1966), p. 4. In a more recent work, Ben-David credits Merton's 1937 paper on "Science and the Social Order" with underscoring the norm-based, collective nature of the "community" of science, and Polanyi in the 1940s and Shils in the 1950s are noted for developing this concept to the extent that it could form the foundation for the formal sociology of science in the 1960s like Storer's work. See Joseph Ben-David, *The Scientist's Role in Society* (Englewood Cliffs, N.J.: Prentice Hall, 1971), pp. 3-4.

[14] Storer, *The Social System of Science*, p. 6.

[15] In addition to Merton's dissertation ("Science, Technology, and Society in Seventeenth-Century England"), completed in 1935, the three relevant papers are: "Science and the Social Order," read at the American Sociological Society Conference in 1937; "Science and Democratic Social Structure," written in 1942; and "The Machine, the Worker, and the Engineer," in 1948. These papers are reprinted in Merton's *Social Theory and Social Structure*, pp. 591-627.

[16] Bernard Barber, *Science and the Social Order* (New York: The Free Press, 1952).

[17] Edward Shils, "Scientific Community:

Kuhn,[18] Kaplan,[19] Barber and Hirsch,[20] Hagstrom,[21] Storer,[22] and Merton[23] advanced the sociology of science paradigm.[24]

A key feature of these works is a conception of science "as a stable set of patterns of interaction, organized about the exchange of a qualitatively unique commodity and guided by a shared set of norms that facilitate the continuing circulation of that commodity."[25] Following from the work of Parsons,[26] Storer identifies "social exchange" as the cement of scientists' interrelationships. Three principles governing exchange relationships within a given scientific system are proposed: (1) desire for a specific commodity, (2) the necessity of exchange as a mechanism for obtaining it, and (3) exchange of the commodity must be met with an "appropriate" (presumably equitable) commodity in return. The fundamental commodities of scientific exchange, contends Storer (and he is echoed by Stein,[27] Price,[28] and Hagstrom[29]), are "creativity" and "response to creativity":

> The terms of exchange in the social system (science) are essentially the offer of one's creative work in return for response, *competent evaluation*, from others. . . . The response to creativity is thus a commodity comparable to goods, power, love and the affirmation of basic values; it may not be of as universal value as are the other four, but so long as creativity is of continuing interest to a substantial number of individuals, it can form the basis of a social system (science) in the same way the other four do (in economic, political, family, and religious systems).[30]

The basic building block of science, then, is "discovery," or the creative generation of relevant information by its scientists. The advancement of any science depends on the reciprocal exchange[31] of "discovered" knowledge for *competent response* from fellow scientists (that is, critical evaluation, followed by utilization and appropriate reward). Most importantly, these exchange relationships, as well as the principles

Thoughts After Hamburg," in Edward Shils, *The Intellectuals and the Powers, Selected Papers*, Vol. 1 (Chicago: University of Chicago Press, 1972), 204-212.

18 Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962).

19 Norman Kaplan, "The Sociology of Science," in Robert E. Faris (Ed.), *Handbook of Modern Sociology* (Chicago: Rand McNally, 1964).

20 Bernard Barber and Walter Hirch (Eds.), *The Sociology of Science* (New York: The Free Press, 1962).

21 Warren O. Hagstrom, *The Scientific Community* (New York: Basic Books, 1965).

22 Storer, *The Social System of Science*.

23 Robert K. Merton (Ed.), *The Sociology of Science: Theoretical and Empirical Investigations* (Chicago: University of Chicago Press, 1973).

24 See, for example, Jonathan Cole and Stephen Cole, *Social Stratification in Science* (Chicago: University of Chicago Press, 1973); Harriet A. Zuckerman, "Patterns of Name Ordering Among Authors of Scientific Papers: A Study of Symbolism and Its Ambiquity," *American Journal of Sociology*, 74 (1968), 276-291; Diana Crane, *Invisible Colleges: Diffusion of Knowledge in Scientific Communities* (Chicago: University of Chicago Press, 1972); Nicholas C. Mullins, "The Distribution of Social and Cultural Properties in Informal Communication Networks among Biological Scientists," *American Sociological Review*, 33 (1968), 768-797; Herbert Menzel, "Scientific Communication: Five Themes from Social Research," *American Psychologist*, 21 (1966), 999-1004.

25 Storer, *The Social System of Science*, p. 75.

26 Talcott Parsons, *The Social System* (New York: The Free Press, 1951). Storer's comparison of the "exchange" concept across social systems, including science, was anticipated by Neil J. Smelser, "A Comparative View of Exchange Systems," *Economic Development and Cultural Change*, 7 (1959), 173-182.

27 Morris I. Stein, "Creativity and the Scientist," in Barber and Hirsch, *The Sociology of Science*, pp. 329-343.

28 Derek J. De Solla Price, *Little Science, Big Science* (New York: Columbia University Press, 1963), pp. 107-108.

29 Hagstrom, *The Scientific Community*, p. 13.

30 Storer, The Social System of Science, p. 76. Parentheses and italics added.

31 On "reciprocity" as normative, see Alvin W. Gouldner, "The Norm of Reciprocity," *American Sociological Review*, 25 (1960) 161-178. Hagstrom sees scientific exchange in terms of "information" and "social recognition": "The organization of science consists of an exchange of social recognition for information." See Hagstrom, *The Scientific Community*, p. 13.

enumerated above which provide for their maintenance and stability, are given practical force by the *norms* of science in general and specific norms operative within a particular scientific discipline.

The existence and nature of scientific norms have been documented and described by Merton in his now classic paper, "Science and Democratic Social Structure."[32] Merton identifies four norms (which he terms "institutional imperatives") as comprising "the ethos of modern science":

(1) *Universalism* implies that scientific methods and knowledge are non-nationalistic, impersonal and catholic. What is scientifically confirmed by one researcher is true irrespective of the researcher's traits or affiliations; "objectivity precludes particularism."[33]

(2) *Communism*, or community, implies a common ownership of scientific knowledge. As Merton notes, "the substantive findings of science are a product of social collaboration and are assigned to the community."[34] Furthermore, Storer observes that this norm fosters an exchange of "creativity" and "competent response" in so far as it "encourages the scientist to take the initiative in placing his findings before his fellow scientists."[35] Merton adds: "Secrecy is the antithesis of this norm; full and open communication its enactment."[36]

(3) *Disinterestedness* proscribes personal gain from one's research and prescribes creative research and knowledge generation as ends unto themselves.

(4) *Organized skepticism* mandates "the suspension of judgment until 'the facts are at hand.'"[37] The scientist must be skeptical of his own research findings (as C. Wright Mills remarked, "To be able to trust yet to be skeptical of your own experience, I have come to believe, is one mark of the mature workman"[38]). And the scientist must scrutinize the work of others. Before basing his own work on another's findings, those results must be critically evaluated and recertified. Further, the scientist is obligated to communicate all criticisms unearthed from application of such skepticism to the researcher in error, the scientific community, and the public if need be.

Two points emerge from the analysis provided thus far. First, critical evaluation is part and parcel of the *modus operandi* of science. Especially when viewed as legitimate and competent response by the community of scientists, criticism functions to preserve the structure of a science community by providing individual reinforcement (positive or negative) for those producing scientists intent on making a creative contribution to the community's knowledge store. Second, criticism can be located within one of the normative bases of science (organized skepticism), a norm to which scientists conform in their personal roles as scientists and in all professional roles they may be called upon to perform. Adherence to this norm also helps insure that the knowledge base of that science is sound (hence a self-preservation function) and that knowledge advances will proceed systematically, as if through a knowledge filtration system of scrutiny of all new discoveries (that is a quality control function).

These three functions of criticism—competent evaluation, self preservation, and quality control—are dependent ini-

[32] Merton, *Social Theory and Social Structure*, pp. 604-615.

[33] Merton, *Social Theory and Social Structure*, p. 607.

[34] Merton, *Social Theory and Social Structure*, p. 610.

[35] Storer, *The Social System of Science*, p. 79.

[36] Merton, *Social Theory and Social Structure*, p. 611. Merton illustrates his point with Huxley's analysis of Henry Cavendish: "Our admiration of his genius is tampered by a certain disapproval; we feel such a man is selfish and anti-social," p. 611.

[37] Merton, *Social Theory and Social Structure*, p. 614. For an interesting treatment of how early American scientists' skepticism about "artful communication . . . gave way to practiced demands for an art of scientific communication," see Joseph W. Wenzel, "Rhetoric and Anti-Rhetoric in Early American Scientific Societies," *Quarterly Journal of Speech*, 60 (1974), 328-336.

[38] C. Wright Mills, *The Sociological Imagination* (New York: Oxford University Press, 1974), p. 197.

tially and primarily upon the *private* application of evaluation criteria to fellow scientists' works. The consequences of this appraisal, whether reward or censure, may be made public of course. But there is nothing inherent in these critical functions which requires more than private exchange of relevant interpretations between the producing scientist and a fellow critic (for example, a helpful colleague or paper reader). These functions do not necessitate public expression of evaluation, except in cases where (1) past errors confound present interpretations, (2) an investigator is intractable in the face of criticism, or (3) the issue is one of major concern to other non-scientific groups, thereby compelling public presentation of the "facts" for debate.[39]

On the other hand, criticism may also be formalized and made *public* in state of the art addresses, annual reviews, synthetic critique articles, and so forth. When public, criticism serves the above three functions *plus* three additional functions for the scientific community. First, public criticism can provide order to divergent researches and establish priorities for the kinds of research efforts most needed. Second, public criticism may provide other scientists with new insights and thus have heuristic value for theory development and future research. Finally, when routinized, such ritualistic criticism reinforces the norms of the scientific community, serves to solidify its members, and gives official notice of the community to other social groups. To the first three functions of private critical evaluation (*competent response, scientific self-preservation,* and *product quality control*), we may therefore add the functions performed by public criticism of communication theory and research: *prioritization, heuristic stimulus,* and *identity validation.* Consider now how these critical functions are instantiated in communication science.

Structures for Criticism in Communication Science

There exist specific checkpoints within and outside the structure of communication science which are intended to safeguard the community and which facilitate its ostensible goals of knowledge generation, prediction, and control of communication-related phenomena. Within the sphere of "private" critical response, there are four groups of persons who serve in critical roles. First, *colleagues* may provide helpful critical insights at any stage of the research process. They may be members of one's immediate research environment, or they may be part of an "invisible college," that widespread network of fellow scientists with whom the researcher communicates regularly but privately.[40] Within this sphere, criticism functions as competent response to one's efforts and preliminary quality control. There are also special *gatekeepers* whose duty it is to critically evaluate a given (academic) scientist's work for purposes of formal competent response and/or quality control. These gatekeepers, arrayed somewhat sequentially, include (1)

[39] Consider the public and scientific debate surrounding such controversial and important issues as recombinant DNA research, the population-famine relationship, and claims of a link between heredity and intelligence. A new forum for critical evaluation has recently been proposed: "the establishment of a kind of 'science court' whose purpose it would be to clarify the controversy clouding a particular scientific issue." The idea entails formation of impartial and distinguished scientist "juries" who would deliver an opinion following courtlike deliberations involving presentation of findings as evidence. The proposal has received support from more than twenty-five heads of science associations. See Amitai Etzioni, "Public Affairs: A Science Court," *Human Behavior,* 6:11 (1977), 18-19.

[40] Diana Crane, *Invisible Colleges; Diffusion of Knowledge in Scientific Communities* (Chicago: University of Chicago Press, 1972).

departmental members who first examine the apprentice's work and hire him/her into the institution, thereby conferring journeyman status within the scientific community; (2) grant proposal readers who determine the merits of both the newcomer's and oldtimers' ideas and decide levels of funding support for test of those ideas; (3) division readers assigned to select final research reports competitively submitted for presentation on the profession's convention panels; (4) the editor and referees of the journals and book publishing institutions the scientist graces with scientific paper submission; (5) specially constituted committees from professional organizations within the science community who recognize scholarly accomplishment; (6) departmental members (including senior scientists) who weigh the results of scholarship for purposes of salary dispositions and promotion recommendations; (7) distinguished members of the scientific community called upon to impartially evaluate a scientist's contributions to the community and provide testimony to (8) representatives of other disciplines (including non-scientific ones) seated on college and university promotion committees who vouchsafe the responses performed at other stages and bestow formal recognition and status on the academic-scientist and help preserve the integrity of the field. Third, and most important, there are the members of the *scientific community* to whom the reports of one's discoveries are addressed, and who critically and privately assess their quality (for example, convention program audiences and journal readers). Should the scientist's work pass their muster, both the scientist and the work are further absorbed into the community mainstream. Finally, there are persons outside the immediate scientific community, *members of other social groups,* such as outside readers of communication journals and books and persons influenced by scientists acting in a consulting capacity, who have contact with the communication scientist's work and evaluate it for personal edification and utilization.

What is common to these groups is the fact that the critical functions they perform usually are non-public. Sometimes the producing scientist may receive informal or formal feedback from members of these groups. More often there is no direct response, but only the utilization of one's findings in others' work. The critical evaluation is intended primarily for the scientist and, while it serves the functions afore mentioned, it is not meant to be public.

Members of these same four groups may also serve as "public" critics of communication theory and research and evince all six critical functions alluded to above. For example, when a scientist presents his work in colloquium fashion to colleagues, their critical commentary takes on a decidedly public flavor intended as much for quality control or heuristic value as for competent response to the presenter. Second, key gatekeepers provide particularly valuable "public" critical functions through state of the art programs;[41] annual reviews such as those in the newly instituted

[41] For example, see the following papers delivered on a "State of the Art" panel sponsored by the Interpersonal Communication Division at the 1977 International Communication Association's conference in Berlin, West Germany: Gerald R. Miller, "Current Perspectives in Interpersonal Communication: Theory and Research"; Arthur P. Bochner, "On Taking Ourselves Seriously: An Analysis of Some Current Trends in Interpersonal Communication Inquiry"; and Dean E. Hewes, "Current Perspectives in Interpersonal Communication: Research Methodology". For a more ambitious attempt at same, see the twenty-seven position papers distributed as part of a program on "The Future of Communication Theory" at the conveniton of the Southern Speech Communication Association in Knoxville, Tennessee, April, 1977.

Communication Yearbooks;[42] editors' judicious appropriation of journal space for review essays,[43] instructive symposia,[44] and "colloquy" dialogues[45]; convention critics' presentations at conferences;[46] and reports from national conferences on priorities in the field.[47] But, third, the most important public criticism is that engaged in by fellow members of the scientific community. The skeptical posture they bring to every research report and their printed "rejoinders" in the journals,[48] as well as their published critical reviews of research,[49] theory,[50] and methods,[51] are the best arbiters of the field's health and status. Finally, the public evaluations supplied by observers, often members of other groups, may both heighten awareness of the communication community by outsiders and solidly its own membership.[52]

[42] See the "overview" by each ICA division head in Brent D. Ruben (Ed.), *Communication Yearbook I* (New Brunswick, N.J.: Transaction Books, 1977).

[43] Especially noteworthy are the series of wide reaching and instructive book review essays published during Gerald R. Miller's 1974-1977 tenure as editor of the then brand new *Human Communication Research* (with the assistance of book review essay editor Donald Cushman) and the "state of the art" reviews initiated thereafter by Miller's successor, James C. McCroskey.

[44] Entire issues in recent *Communication Quarterly* have been devoted to symposia on "Naturalistic Study of Communication," 25 (Summer, 1977) and "Alternative Theoretical Bases for the Study of Human Communication," 25 (Winter, 1977). See also "Symposium: What Criteria Should Be Used to Judge the Admissibility of Evidence to Support Theoretical Propositions in Communication Research?" *Western Journal of Speech Communication*, 41 (Winter, 1977), and the entire 42 (Winter, 1978) issue of the journal devoted to responses to that symposium. Too, see the selected symposia on TV news, media and medicine, TV effects, government and media, paranormal communication, and cultural exchange in *Journal of Communication* beginning with George Gerbner's editorship in 1974.

[45] See the "Colloquy" department of *Quarterly Journal of Speech* inaugurated in the February, 1975 issue by the incoming editor, Edwin Black. The exchange between Hawes and co-authors Grossberg and O'Keefe in the next issue's Colloquy section exemplifies the kind of public "critical response" I have been proposing as an essential expression of criticism of communication theory and research. See Lawrence Grossberg and Daniel J. O'Keefe, "Presuppositions, Conceptual Foundations, and Communication Theory: On Hawes' Approach to Communication," *Quarterly Journal of Speech*, 61 (1975), 195-208, and response by Leonard C. Hawes, "A Response to Grossberg and O'Keefe: Building a Human Science of Communication," pp. 209-219 of the same issue. Therefore, their debate shifted to the shorter "*Forum*" section *QJS*, 61 (Oct. 1975). The correct order of their rejoinders should be O'Keefe and Grossberg, "Theoretical Clarity and Interpretive Social Science," pp. 320-322, and *then* Hawes, "Theoretical Pragmatics and Scholastic Exegetics, or Will the 'Real' Schutz and Brodbeck Please Sit Down," pp. 319-320.

[46] David R. Seibold, "(Top Three Plus Two) + (One Critic) = A Critique of ISGC's Finest," Paper presented to the Interpersonal and Small Group Communication Division of the Speech Communication Association, Washington, D.C., December, 1977.

[47] See, for example, the summary of recommendations by the Speech Communication Association's Conference on Long-Range Goals reported in *Spectra*, 8 (Decemeber, 1972), 2-3.

[48] Illustrative is W. Barnett Pearce, "Consensual Rules in Interpersonal Communication: A Reply to Cushman and Whiting," *Journal of Communication*, 23 (1973), 160-168.

[49] Especially noteworthy are Jesse G. Delia, "A Constructivist Analysis of the Concept of Credibility," *Quarterly Journal of Speech*, 62 (1976), 361-375, and Donald A. Clement and Kenneth D. Frandsen, "On Conceptual and Empirical Treatments of Feedback in Human Communication," *Communication Monographs*, 43 (1976), 11-28.

[50] See Dennis S. Gouran, "Perspectives on the Study of Leadership: Its Present and Its Future," *Quarterly Journal of Speech*, 60 (1974), 376-381; Gerald R. Miller, "Counterattitudinal Advocacy: A Current Appraisal," in C. David Mortensen and Kenneth K. Sereno (Eds.), *Advances In Communication Research* (New York: Harper & Row, 1973), pp. 105-152; and David R. Seibold, "Communication Research and the Attitude-Verbal Report-Overt Behavior Relationship: A Critique and Theoretic Reformulation," *Human Communication Research*, 2 (1975), 3-32.

[51] Lawrence J. Chase and Raymond K. Tucker, "A Power-Analytic Examination of Contemporary Communication Research," *Speech Monographs*, 42 (1975), 29-41; Gary Cronkhite and Jo Liska, "A Critique of Factor Analytic Approaches to the Study of Credibility," *Communication Monographs*, 43 (1976), 91-107; and Joseph N. Cappella, "Research Methodology in Communication: Review and Commentary," in Brent D. Ruben (Ed.), *Communication Yearbook I* (New Brunswick, N.J.: Transaction Books, 1977), pp. 37-54.

[52] For example, consider that *Human Behavior*, a popular press newsmagazine of the social sciences, has begun to present synopses

The *structures* and *functions* of criticism in communication science can thus be summarized in the Criticism Matrix depicted in Figure 1. It should be clear from this diagram, and the analysis from which it is drawn, that an adequate structure exists for instantiation of the six critical functions explicated earlier. In addition, the examples supplied demonstrate that the functions are being met within the existing structure. In fact, the examples cited suggest that there has been an increase in critical response to communication theory and research recently. If the growth of science is exponential, perhaps the same is true for scientific *criticism*, and we may be seeing the rapid acceleration which precedes stabilization in the later stages of the curve.[53] However, recall that only 6.5% of publication space in our field during a five year period was devoted to critical response to the *status quo* in communication. The critical rate was not much higher in the APA sponsored *American Psychologist, Journal of Personality and Social Psychology, Journal of Experimental Psychology,* and *Psychological Bulletin;* it was slightly more than 7% of some 23,000 pages. But the vigor reflected in the 16% critical rate in the ASA journals should spur us to sharpen the role of criticism in communication theory and research, to improve the structure for critical expression in communication science, and to increase our collective critical response to existing theory and research in communication. The final section of this paper follows from the

of communication scientists' work on a monthly basis in the "Communications" department of that periodical. Also see "Colloquy," *Journal of Communication*, 26 (1976), 218-220 for a vivid example of how observers become "critics" of communication science based on reports of research through the mass media.

[53] See Price's interesing discussion of the growth rate of science in *Little Science, Big Science*, pp. 1-32.

FUNCTIONS

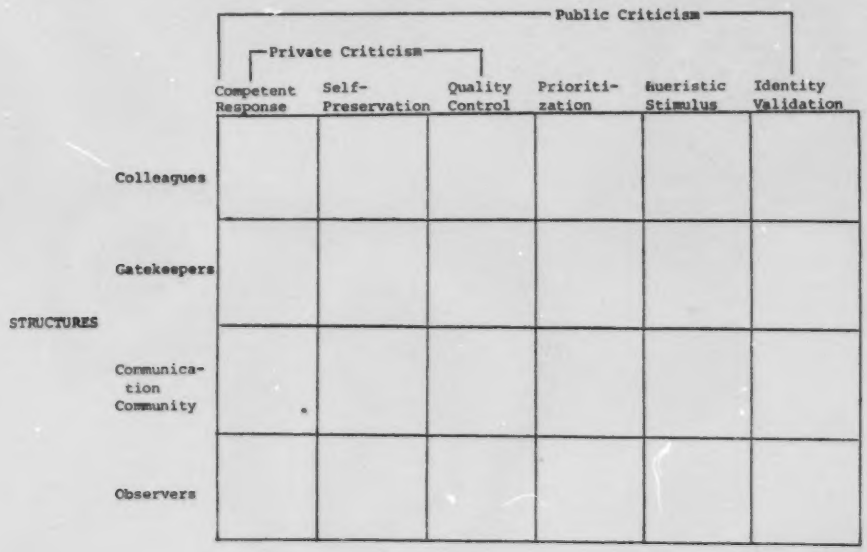

FIGURE 1

CRITICISM MATRIX

preceding "functions" and "structures" analysis of communication science by proposing how those functions may better be met and how the structures for criticism can be improved.

Developing A Critical Attitude in Communication Science

If communication theory and research have suffered a paucity of just allocation of competent response, it is primarily for want of an overarching "critical attitude" in the field of communication. Simply put, most members of the scientific community have been remiss in their obligations to exrcise and encourage the critical functions so vital to communication science: competent evaluation, scrutiny of the findings which form the bedrock of the discipline, and continuing quality control. Too few communication scientists read even the majority of reports published by fellow researchers. Reports that are read typically are skimmed for their "results," findings that too frequently are accepted *privately* at face value. Small wonder that no more than 6.5% of the pages published in our journals reflected *public* criticism of the premises or particulars of previous publications. If those critical functions which safeguard science are to be enacted through the critical structures of communication science (as depicted in the Criticism Matrix), then a more formal, organized skepticism must be cultivated about one's role as a scientist, the objects and purposes of communication research, and especially the knowledge claims on which the discipline is based. As in the sense underscored by Alfred Schutz, adoption of such a "critical attitude" entails interpretation of the goodness of fit between facts or findings, consensually accepted scientific knowledge, and what is known intersubjectively.[54]

How is this critical attitude to be developed and formalized? To begin, a critical attitude must be inculcated at the graduate level. Students, upon whom the future of any field rests, should be (1) exposed formally to specific examples of high and low quality research reports and instructors' carefully developed arguments for why those designations are warranted; (2) required to undertake critical analyses of the general state of the art, independent of their own specific lines of research; and (3) expected to critique their colleagues' and professors' research endeavors, persons likely to serve as (overly) significant models if viewed uncritically. Without this early nurturance, we can expect little change in the field's primary response to convention papers and published reports: lack of awareness, apathetic acceptance, or blind loyalty.

But a critical attitude must transcend the private application of scientific skepticism by apprentices and the bulk of the communication community. In particular, special status must be accorded those select persons who might be termed "weedpullers," or critics from within or outside the scientific community who make public their scrutiny of the field's findings and fallacies. In addition to critical rejoinders to specific articles that contain errors in assump-

[54] Although the term "critical attitude" is not explicitly used by Schutz, he refers to what Swanson, "A Reflective View . . .," pp. 209-210, has labeled the "critical stance" of the "natural attitude" in Schutz's description of the "scientific attitude" and the interpretive activity of scientists. I have concatenated Swanson's term and Schutz's sense of the term into "critical attitude." See Alfred Schutz, *Alfred Schutz on Phenomenology and Social Relations: Selected Writings*, ed. and intro. by Helmut R. Wagner (Chicago: U. of Chicago Press, 1970), pp. 275-278; and Alfred Schutz, *The Phenomenology of the Social World*, trans. by George Walsh and Frederick Lehnert (Evanston, Ill.: Northwestern University Press, 1967), p. 10.

tions, methods, or interpretations, weed-pullers would provide focused critiques of entire lines of research. For example, communication science has recently witnessed a proliferation of self-report instruments designed to measure traits or states related to what I shall call, generally, "Communicator Orientation," or communicators' personal feelings and expectations about interaction. A weed-puller would attempt to draw clear distinctions among potential dimensions of Communicator Orientation; how are Predispositions Toward Verbal Behavior, Rhetorical Sensitivity, Communicator Style, Introversion, Reticence, Personal Orientations Toward Communication, Communicator Apprehension, Uwillingness To Communicate, Trust, Powerlessness, Social Isolation, Self Esteem, and Expressed Control all conceptually and empirically related? More important, what are the bases, or antecedents, to their relationships within the CO rubric? And how could the socially-based intrapersonal dynamics implied by symbolic interactionism, developmental theories, constructivism, learning theory, psychodynamic theories, or interactional psychology inform our understanding of CO and supply a foundation for a substantive "theory" of Communicator Orientation (that is, how do these frameworks help delineate the processes by which persons come to develop orientations toward communication along any of the nonredundant dimensions above)? Finally, what is the relationship between Communication Orientation and the theoretical morass now called "communication competence" (that is, how does CO explain and predict successful, adaptive interaction)?

The private and public application of a critical stance must not be restricted to members of the communication community only. It must be complemented by critiques from (1) scientists outside communication and (2) "applied" professionals. In the first area, scientists from other disciplines should routinely be invited to serve as critics on convention panels related to their expertise, to provide summary critiques of specific reviews in the annual *Communication Yearbook*, and to briefly respond to the comprehensiveness, interpretations, and recommendations of state of the art reviews in *Human Communication Research*.[55] Psychologist Percy Tannenbaum vigorously exercised his role as critic at an International Communication Association convention several years ago by concluding that, despite the significant perspective communication *could* supply, research to that point on resistance to persuasion by communication researchers was overly restrictive modeling of psychologists' work in the area. And he was right, then! As in business, sometimes it is the invited consultant who can produce the quickest turnabout. In the second area, professionals from government industry, civic affairs, medicine, and so forth should be invited to respond to how well current research directed toward them actually meets their needs. For example, several doctors with whom I have spoken have expressed great interest in current communication research on doctor-patient interaction, but also identified aspects of that research which seemed under-researched from their perspective. Criticisms like these (if made public through any of the structures mentioned above or through established liasons with other disciplines, communication organizations outide the United States, or government and business) might help order research priorities in

[55] Alternatively, the Book Review Editor of *QJS* or *CE* might invite an outside expert to write a major review essay on all the recent books in a given area (e.g., small group, interpersonal, or nonverbal communication) written by communication scientists.

what are primarily applied research areas, provide stimuli for different research, and improve relations between the research community and its "consumers."

Since public expressions of the critical attitude will necessarily involve our journals, it also may be important to scrutinize their role for communication scientists. Journals are probably the major repository of our field's knowledge, they represent the most direct means of communicating new findings to the rest of the communication community, and, by virtue of their very limited number and space, publishing in a journal serves as a basis for direct and indirect rewards, achievement, and movement within the strata of the scientific community.[56] As important as journals are to the field of communication, there has been very little systematic scrutiny of their operation. We know that the rejection rate for manuscripts submissed has ranged from 74 percent to 86 percent in *Communication Education* (74%), *Communication Monographs* (80%), *Quarterly Journal of Speech* (86%) and *Human Communication Research* (84%).[57] Regular contributors, especially, develop an "informal" network and insight into the publication process, but they are a minority of the field (in fact, I doubt that more than five percent of those members now in the field have published two or more articles). Little attempt has been made to hold up the entire journal structure for analysis by the entire communication community. By contrast, psychologists and sociologists have undertaken careful study of their research outlets, including the use of standardized evaluation forms—especially referees' willingness to use a standardized rating form for empirical manuscripts,[58] degree of consensus among editors and referees about suitable criteria for a standard evaluation form,[59] the reliability with which a standardized rating form can be applied;[60] general screening procedures for articles—especially the reliability of referees' judgment,[61] favoritism,[62] editor-author background similarity,[63] and suggestions for improv-

[56] See Duncan Lindsey, "Distinction, Achievement, and Editorial Board Membership," *American Psychologist*, 31 (1976), 799-804 for a discussion of the "knowledge filtration system" supplied by journals and the social stratification system of a science, and Harriet Zuckerman "Stratification in American Science," *Sociological Inquiry*, 40 (1970), 235-257. For the relationship between publishing and "rewards," see Bernard H. Gustin, "Charisma, Recognition, and the Motivation of Scientists," *American Journal of Sociology*, 78 (1973), 1119-1134; C. Rogers Myers, "Journal Citations and Scientific Eminence in Contemporary Psychology," *American Psychologist*, 25 (1970), 1041-1048; and Stephen Cole and Jonathan R. Cole, "Scientific Output and Recognition: A Study in the Operation of the Reward System in Science," *American Sociological Review*, 32 (1967), 377-390.

[57] These rejection rates (based on ratios of only those articles fully accepted to all submitted) are based upon each editor's report to the Speech Communication Association Legislative Council in 1977 and a chart reported in "A Note From the Editor," *Communication Research*, 4 (1978), 195. It would be interesting to compare these rates with those in the four regional SCA journals.

[58] Wirt M. Wolff, "Publication Problems in Psychology and an Explicit Evaluation Schema for Manuscripts," *American Psychologist*, 28 (1973), 257-261.

[59] Wirt M. Wolff, "A Study of Criteria for Journal Manuscripts," *American Psychologist*, 25 (1970), 636-639, and Janet M. Chase, "Normative Criteria for Scientific Publication," *American Sociologist*, 5 (1970), 262-265.

[60] See Paul McReynolds, "Reliability of Ratings of Research Papers," *American Psychologist*, 26 (1971), 400-401; William A. Scott, "Interreferee Agreement on Some Characteristics of Manuscripts Submitted to the *Journal of Personality and Social Psychology*," *American Psychologist*, 29 (1974), 689-702; and Donald D. Bowen, Robert Perloff, and Jacob Jacoby, "Improving Manuscript Evaluation Procedures," *American Psychologist*, 27 (1972), 221-225.

[61] R. G. Kinkade, "The Experimental Publication System: An Aid to Information System Planning," *American Psychologist*, 25 (1970), 925-936.

[62] Dirk L. Schaeffer, "Do APA Journals Play Professional Favorites?" *American Psychologist*, 25 (1970), 362-365.

[63] See D. Tasto, G. Dinges, R. Weigel, and H. K. Hulse, "Institutional Ties of Authors and Editors of the *Journal of Counseling Psychology*." Paper presented at the meetings of

ing the review process;[64] criteria employed in selection of editorial board members;[65] and comparison of editorial policies and practices in their journals to those in other fields.[66]

In short, part of a critical attitude in communication science involves examining the structure of that system (especially its gatekeepers), not simply its products. This means analysis of the criteria referees utilize (there always seems to be just enough reliability in their final judgments to think they are providing competent response, but not sufficient agreement to trust that the bases for their response are common); deliberation on what should count as scientific "evidence"; standards for the reliability and validity of scales and instruments and the power of statistical tests; need for replication of original findings; statistics which must accompany each report (especially a correlation or variance-covariance matrix); and so forth. Too, editors should use their own journals, or be provided space in *Spectra*, to discuss the many aspects of their position which affect the publication of research (e.g., selection of editorial boards, preliminary screening of articles, choice of referees for specific submissions, breaking ties among referees' recommendations, encouraging critical rejoinders to published articles, etc.).[67] Finally, alternative channels for the public expression of critical attitude must be sought (for example, division newsletters, guest editorials in *Spectra*, separate journal space for unsuccessful replication research and comparisons of field with laboratory findings, and new journals).

The preceding arguments for the importance of critical attitude in communication science presume that this formal, organized skepticism will proceed hand-in-hand with the other norms of science, including universalism and disinterestedness—both of which proscribe scientific judgments based on particularistic or ascriptive criteria determined by the judge's personal biases. But, as Miller and Berger have noted recently, an ecumenical spirit does not exist within the communication science community; groups of cultists have been preaching particularistic gospels.[68] This probably is not bad for the discipline, *per se*, for if we have learned nothing else from the history of science it is that a science cannot progress unless its proponents are willing to accept new positions and change. What is unfortunate is that the

the Rocky Mountain Psychological Association, Denver, May, 1971. Also see Diana Crane, "The Gatekeepers of Science: Some Factors Affecting the Selection of Articles for Scientific Journals," *American Sociologist*, 32 (1967), 195-201.

[64] Yvonne Brackbill and Frances Korten, "Journal Reviewing Practices: Authors' and APA Members' Suggestions for Revision," *American Psychologist*, 25 (1970), 937-940; S. H. Newman, "Improving the Evaluation of Submitted Manuscripts," *American Psychologist*, 21 (1966), 980-981.

[65] See Duncan Lindsey, "Distinction, Achievement and Editorial Board Membership," *op. cit.*, and William C. Yoels, "The Structure of Scientific Fields and the Allocation of Editorships on Scientific Journals: Some Observations on the Politics of Knowledge," *Sociological Quarterly*, 15 (1974), 264-276.

[66] Janice M. Beyer, "Editorial Policies and Practices Among Leading Journals in Four Scientific Fields," *Sociological Quarterly*, 19 (1978), 68-88; Jeffrey Pfeffer, Anthony Leong, and Katherine Strehl, "Paradigm Development and Particularism: Journal Publication in Three Scientific Disciplines," *Social Forces*, 51 (1977), 938-951; and Harriet Zuckerman and Robert K. Merton, "Patterns of Evaluation in Science: Institutionalism, Structure and Functions of the Referee System," *Minerva*, 9 (1971), 66-100.

[67] See, for example, "Publication in APA Journals: Some Advice from the Editors," *American Psychologist*, 20 (1965), 711-720; and Hyman Rodman, "Notes to an Incoming Journal Editor," *American Psychologist*, 25 (1970), 269-273.

[68] Gerald R. Miller and Charles R. Berger, "On Keeping the Faith in Matters Scientific," *Western Journal of Speech Communication*, 42 (1978), 44-57. Alfred McClung Lee has noted the same tendency toward "politicization" of competing groups in sociology and its detrimental effects; see "Presidential Address: Sociology for Whom?" *American Sociological Review*, 41 (1976), 925-936.

disciples of each new testament have not examined their own tenets as critically as they have scrutinized others' views. Formalization of the "critical attitude" proposed here proscribes blind loyalty and presupposes equanimous application of skepticism to all scientific positions: past and present, others' and one's own. F. P. Dunne captured the sentiment of this critical attitude with the enjoinder to "Trust everybody, but cut the cards."

FUNDAMENTAL ISSUES IN THE CRITICISM OF COMMUNICATION THEORY AND RESEARCH

Dennis S. Gouran

ALTHOUGH the type of scholarship commonly referred to as "communication theory and research" suffers from no want of appraisal, the role such criticism plays has yet to become well defined. Let me acknowledge at the outset of this essay that the expression "communication theory and research" refers to inquiry conducted within the discipline having a purportedly scientific character. This is not to say that criticism should be restricted to only this area of the field, nor is it to ignore the considerable body of scholarship published in rhetorical and dramatic criticism. The point to be emphasized is that within the branch of the discipline called "communication theory and research," criticism has no clearly articulated foundation that contributes to one's understanding of its role. Since science is a uniquely human activity, however, there is no necessary reason that it should be less an object of formal evaluation than any other type of human activity. In fact, as I shall argue, there are good reasons why it must be subject to critical scrutiny.

The causes underlying the failure to establish the role of criticism in communication theory and research are undoubtedly varied and complex. One possible contributing factor, however, is the notion that science is a self-validating process. Conformity to the rules on which it is based, then, would appear to be sufficient for determining the acceptability of conclusions to which scientific inquiry leads and the conditions under which such conclusions are produced. But even if this kind of idealization of the enterprise were a reality, the problem of knowing whether the rules in any given case have been followed and how faithfully would still remain. This type of determination, I submit, is judgmental, and, therefore, even the idealization of scientific activity does not preclude the need for criticism.

It would be an act of willful blindness to suggest that criticism currently plays no role in communication theory and research; however, the role it does play is not particularly clear or well founded on an acknowledged body of principle. In short, criticism does not appear to be a crucial factor in the science of communicative behavior. That it should be I hope to demonstrate in the remainder of this essay. For the moment, however, it might be well to point out that criticism can be a determinant and a reflection of a discipline's maturity and status. As Eisenstadt and Curelaru have observed in the case of sociology, that discipline's emergence as a distinctive field, in part, occurred when sociologists began to address research problems

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"with the methods of scientific scholarly criticism."[1]

To invest the role of criticism with better definition requires an examination of the functions it presently serves vis a vis those it needs to serve. This I take to be the fundamental issue that must be resolved if criticism is to become a more meaningful part of the total enterprise of communication theory and research. Before addressing this issue, it would be well to indicate what I mean by criticism. I should first make it clear, however, that the definition of the term is not what I mean by definition of the role—although the former may prove useful in interpreting the latter.

According to Black, "Criticism is a discipline, that through the investigation and appraisal of the activities of man, seeks as its end the understanding of man himself."[2] My initial impulse is to seize upon this well known definition, declare it applicability to the disciplined evaluation of any type of systematic human activity, and be on with the business of exploring related problems. Unfortunately, the definition, upon closer scrutiny, proves somewhat inadequate for purposes of this inquiry, for it seems to establish criticism as the counterpart of science. In other words, it suggests an alternative way of arriving at an understanding of human behavior. That such alternatives exist is not a matter of contention, but the aim of criticism I have in mind is not to produce understanding of behavior as such. Rather, it is to determine whether individuals investigating it scientifically can legitimately claim that they have added to our understanding. As a result, I shall define criticism as *the systematic assessment of the conditions under which a knowledge claim has been produced and the subsequent determination of its acceptability.*

Within this frame of reference, let me now identify the functions criticism in communication theory and research presently serves. Discussion of this issue, of course, is complicated by the fact that so much of the criticism that takes place is not in the public domain. Rather, it occurs in the form of private exchanges between, say, an editor and author, teacher and student, or researcher and colleague. In spite of this limitation, one can recognize several functions that criticism as practiced appears to serve.

In my more cynical moments, I would argue that the functions of criticism are three: clarifying the stylistic requirements of professional journals, amusing audiences who attend convention programs, and infuriating authors of manuscripts rejected for publication. In more reasonable moments, however, I would be constrained to acknowledge that these factors, at best, are incidental to the critical process. Among those functions criticism apparently is intended to serve, the principal one currently seems to be screening. Editorial boards of professional journals are assembled for the purpose of discovering the strengths and weaknesses of manuscripts submitted for consideration and possible publication by an individual serving as editor. Associates are appointed on the basis of their presumed or recognized expertise to advise and assist the editor in making publication decisions. In so doing, the associate or consulting editor usually has free rein in making a recommendation, which the editor can subsequently choose to follow or ignore. A publication recommendation may or may not be the result of careful assessment of the legitimacy of knowledge claims. In general, however, I would assume that

[1] S. N. Eisenstadt and Curelaru, *The Form of Sociology: Paradigms and Crises* (New York: Wiley, 1976), p. 16.

[2] Edwin Black, *Rhetorical Criticism: A Study in Method* (New York: Macmillan, 1965), p. 9.

most people involved in the screening process are conscientious and try to be as fair as possible within the limits of their capabilities in considering the merits of the manuscripts they review. I would further assume that, with notable exceptions, most make some effort to reveal the criteria on which their recommendations are based.

If there is a problem associated with the exercise of the screening function, it is probably attributable to the range of standards represented by the diverse composition within and among editorial staffs. As a result, it is entirely possible for one to receive the following set of comments:

Critic A: "The study is conceptually sound but methodologically weak."
Critic B: "This study is methodologically very sophisticated but conceptually indefensible."
Editor: "As you can tell from the critics' comments, both perceive serious weaknesses in your study. I concur in their judgment."

Perplexing as this type of inconsistency may be, it does not make the screening function any less important or desirable. It does raise questions, however, about the soundness of judgment exercised.

From some points of view, the previously mentioned problem is less serious to the extent that one's opportunity to be heard by the scholarly community is not foreclosed by one unfavorable publication decision. As Kaplan puts it, "The power structure in science can be relied on to serve the general welfare of the scientific community only if they are subject to some system, however informal, of checks and balances, so that what is rejected by one journal or professional association may find acceptance in another."[3] Although one can easily agree with this observation, reliance on such a possibility is not a sufficient response to the problem of inconsistent judgment.

A second function that criticism in communication theory and research serves is providing bases for the generation of new research. As a result of some perceived difficulty in a published study, or as a result of the logical implications of a particular set of findings, an individual may be led to some new line of investigation concerning a communication problem of interest. Assume, for example, that one were to encounter a study comparing the relative effectiveness of the discussion and lecture methods of instruction on information acquisition. Further assume a set of findings that indicate retention of the null hypothesis. A discerning reader might criticize such a study for its lack of control of the ability levels of the experimental subjects. Arguing on the basis of previous research that might have shown that high ability students prefer the discussion method whereas those low in ability prefer the lecture method, this hypothetical investigator could reason that preferences affect motivation and therefore hypothesize an interaction between method of instruction and ability level, which he or she then proceeds to test. Rather than concluding that method of instruction has no effect, then, one might discover that method is a crucial factor in determining what one learns.

Occasionally, we observe the results of the exercise of the type of critical function the previous illustration suggests. As often as not, however, the act of criticism is reduced to the observation that previous researchers have failed to do the study an investigator has undertaken. The new study consequently is justified because no one else has done it. The supressed premise in this type of logical construction is an interesting one. Anything not done is worth doing. For

[3] Abraham Kaplan, *The Conduct of Inquiry* (San Francisco: Chandler, 1964), p. 376.

anyone accepting the premise, the perception that a study has not been done previously has a strong legitimizing influence for undertaking the inquiry. In any other sense, the observation seems to be an absurdity. At the very least, it suggests a value that is difficult to defend.

Using criticism as a basis for generating new research that has some promise of throwing into question the conclusions of previous scholarship, or even strengthening those conclusions, is unquestionably a valuable function. Still this type of activity may not be the most important function criticism conceivably could serve. The type that possibly comes closest to the aim of the previously stated definition is reflected in the critical summary of published theory and research. Our professional journals often contain such reviews. The purpose of these articles ostensibly is to synthesize a body of scholarship, to extract the set of generalizations the synthesis suggests, and to qualify those generalizations in light of any limitations or weaknesses the reviewer may have identified.

Unfortunately, these kinds of exercises can turn out to be more summary than criticism, but the form of development nonetheless seems to have legitimizing impact on the conclusions drawn. We like our knowledge compact, and the critical summary is one vehicle by which this desired state of affairs can be created. One can admit to a certain logic that the cumulative effect of study piled on study might suggest. If many studies arrive at the same conclusions, is it not more likely that the conclusions are true? Not necessarily. A great deal has to do with the conceptualization of research problems and the theoretical premises on which they are based. We have literally hundreds, if not thousands, of investigations, for example, showing that very short experimental messages produce significant attitude change on controversial issues. I have long doubted that this is so and suspect that many research subjects are an attitude scale that is issue related as a rating scale to provide indications of perceived quality. A suspicion, of course, does not mean that one is correct. The point I am trying to make, however, is that cumulative evidence does not reduce the burden of judgment, but some so-called critical summaries do not appear to reflect an appreciation of that reality.

In spite of the kinds of attendant problems noted, the functions of screening, generating new research, and identifying the generalizations warranted by a body of research and theory strike me as eminently sensible and valuable. Certainly, I would be one of the last to recommend their discontinuation. Nevertheless, even taken as a whole, these three functions do not give criticism an adequately defined role in communication theory and research. I believe that there are at least two additional functions criticism should serve. Before I can discuss either of these, however, it is necessary to consider precisely what it is that the scientific approach to communication theory and research does, for a recognition of what this type of activity entails suggests what are, from my perspective, probably the most vital functions criticism can serve.

What is it, then, that the science of communication does? I suspect that most of us would like to make the most substantial claims we can; for example, it betters the human condition, it leads us to ever closer approximations of the truth, or it enables us to control our environment and, hence, our destiny. One may have serious reservations about the legitimacy of such claims, but even if they were unquestionably justified, they would be consequences, not rep-

resentations, of what science does. What science as practiced in our discipline does, in my judgment, is to permit the construction of ampliative statements or inferences with some measurable degree of confidence that they are true. How we interpret the significance of those statements or what uses we make of them are not as such intrinsic to the process that produces them. Although I am expressing a judgment, observation of the contents of our scientific reports would appear to lend some foundation to it.

An ampliative statement, according to Salmon, is one of a class of nondemonstrative inferences that "has a conclusion with content not present either explicitly or implicitly in the premises."[4] Practically any, if not all, instances of inductive inference would serve to illustrate this type of statement. More important than illustration, however, is the question of whether the kind of scientific activity in which we engage leads to these sorts of statements. As nearly as I can determine, they are the only variety of statements to which our scholarship, in fact, can or does lead.

The characteristic form of inquiry in communication theory and research is to infer from a sample of measures population characteristics that have not been, or cannot be, observed in their totality. The statement that "the greater the pressure exerted by the members of a group on an individual to conform to the group's norms, the greater will be the change in the direction of conformity in most instances," for example, would be both ampliative and nondemonstrative because we are not ever in a position to observe all individuals on whom a group might exert pressure. One cannot say, therefore, that the statement is true. As Salmon points out, since the premises of nondemonstrative inferences do not necessitate their conclusions,[5] at best, one hopes to draw inferences whose premises lend weight to their probability values.

Some individuals might characterize scientific inquiry in communication as an essentially deductive process. In other words, one begins with a set of relationships presumed to be true, which in proper juxtaposition imply conclusions that can be tested. A positive result, then, serves a truth preserving function. At this point in our history, there appears to be considerable sentiment for this particular view and corresponding pressure to design studies from such a perspective. As a frequent victim of the consequences of failure to adhere to this frame of reference, let me express two reservations about its significance. First, the relationships which constitute the premises from which research hypotheses are deduced are often themselves hypotheses or, worse, thinly disguised appeals to authority. Given the hypothetical, probabilistic, or authority based character of those premises, a research hypothesis can be supported even though the premises be in error. Second, the research findings generated by this kind of model of necessity are probable and, hence, nondemonstrative. The premises and hypotheses are stated categorically if one is using a truly deductive form, but the confirmatory evidence is statistical. Consequently, the kinds of inferences the deductive model as employed permits are still ampliative and nondemonstrative.

I do not wish to convey the impression that deduction is unimportant. It can be a very useful tool in identifying logical possibilities concerning the nature of the relationships among research variables,

[4] Wesley Salmon, *The Foundations of Scientific Inference* (Pittsburgh: University Press, 1966), p. 8.

[5] Ibid., p. 9.

but one should not be misled by this index of utility. Deduction is not the process by which substance is invested in a knowledge claim. The process in evidence is clearly inductive. It is the process by which research hypotheses are suggested that is sometimes deductive, or hypothetico-deductive to be precise,[6] but not the process by which they are converted to conclusions.

If one accepts the argument that what scientific investigation in communication theory and research does is to enable us to make ampliative inferences, then one of the desirable functions of criticism begins to become apparent. Since the types of inferences we make are inherently probabilistic, it would seem to follow that we want to be reasonably sure that the probabilities investigators report are indeed suggested by data rather than in spite of them and that they are not attributable to factors or influences other than the variables under investigation. An ideal state of assurance concerning these matters is most likely not possible, but that should not preclude the effort to eliminate other explanations for the probabilities research establishes. A scientist has a difficult enough time making the inferential leap from sample to population without the added encumbrances of inaccuracy and artifact.

Much of the problem of determining the accuracy of probability statements has been conveniently resolved by statisticians. The critic need only familiarize himself or herself with and understand the established rules in order to render a critical judgment about the accuracy of a probability statement. I am not referring to the accuracy of the inference but the probability on which it is based. This is a matter of computation, not interpretation. There is another sense in which the accuracy of probability statements can be problematic, however, but for which a knowledge of computational rules is not that helpful. It is in this sense that the critic can perform a useful, if not vital, service.

Consider the case in which an investigator reports a statistically significant difference in the amount of attitude change produced by two individuals of varying credibility. The investigator informs the scholarly community that high credibility leads to positive attitude change and that low credibility does not, or that low credibility is a negative factor in inducing attitude change. The statistical test, however, may not have actually established the conclusion as probable but only that the probability of detecting a difference of the magnitude reported in the study is high. Now further assume that inspection of the data reveals that twenty-five percent of the subjects in the high credibility condition. This fact would suggest that attitude change is more improbable than probable under either condition of source credibility, but that is not what our hypothetical investigator infers from the data. The statistical difference does not warrant the conclusion that high credibility produces attitude change and that low credibility does not. Attitude change is associated with both levels of credibility but is not particularly substantial in either case. What the investigator in the illustration has done is to use one probability estimate as an index of another. One need not peruse the contents of many research reports before discovering an actual example of this type. The misrepresentation, I suspect, is not intentional but merely the manifestation of conventions to which scholars conform in making statements about relationships among research variables. Convention, however, does not excuse misrepresentation.

[6] Kaplan, pp. 9-11.

A second problem associated with ampliative statements involves the conditions under which probability estimates are produced. The two sides of even a biased coin have probabilities that can be calculated, but the question is whether or not we can use such estimates to make claims about other coins. I am not concerned with the problem of bias per se but with the more general one of artifact. If a statistician seeks to rule out chance as an explanation for observed relationships, it seems to me that a critic may want to perform a similar service in relationship to the features of the conditions under which scientific inquiry is conducted.

One of the most impressive criticisms I have ever read is Roger Brown's assessment of research on the theory of cognitive dissonance.[7] A discerning observer, Brown notes that subject loss in some of the experimental studies was typically greatest in those conditions favoring a dissonance theory prediction of attitude change. The effect of such loss would likely be to increase the magnitude of the average shift in attitude in the condition in which it occurs beyond what could be expected had attrition not occurred. If so, then the finding might be more easily attributable to subject loss than to the operation of the research variables under investigation.

Other kinds of artifactual findings may be the product of any number of different factors, such as demand characteristics, inappropriate units of measurement, experimenter bias, subject awareness, and the like. We are, I suspect, generally familiar with the presumed effects these sources of influence have on the outcomes of scientific inquiry, and we even occasionally point to the possibility that one or more of them may have been operative in given instances. I do wonder, however, whether there exists a general standard governing the appropriateness of references to their potential effects in the formation of a critical judgment. If there were such a standard, it might be stated as follows: *The task of criticism in those instances in which the results of an investigation appear to be attributable to factors other than the relationships among the research variables of interact is to determine the potential source of influence and to demonstrate how its manifestation is likely to be responsible for the results obtained.*

The type of standard I am proposing places a burden of proof on the critic that heretofore has not existed—or, at least, seems not to have existed. The principle requires that a critical judgment carry with it the obligation of arguing rather than merely asserting that an alternative explanation involving unwanted sources of influence is tenable. All too often, I have heard someone simply raise the possibility of artifactual influences and then conclude that the results of an investigation are therefore invalid. Even though the critic might be correct about the validity of the findings, it does not necessarily follow that the factor identified can or does operate in the way the critic has implied. I have heard it alleged, for example, on more than one occasion at professional meetings and elsewhere that low reliability in a set of measures can account for a statistically significant finding. While I would not question the relationship between reliability and validity, I have always wondered how those who make this point rationalize the fact that, as the reliability of a measure decreases, measurements approach a condition comparable to what one could expect from random assign-

[7] See Roger Brown, *Social Psychology* (New York: Free Press, 1965), pp. 601-604.

ment, with the fact that the results are statistically significant. A statistically significant finding is less likely, not more likely, when measurement does not improve on chance assignment of values to objects. The assertion on which the example is based is tantamount to arguing that an increase in the probability of a Type II error increases the probability of a Type I error, which, at best, is a logically incompatible set of statistical possibilities. Along the same lines, I have also been curious about why the question of the power of a statistical test is frequently raised by critics in those instances in which a null hypothesis has been rejected, since the power issue has to do with the probability that one falsely retains the hypothesis.

I am not trying to minimize the importance of raising questions about potential sources of error. On the contrary, I believe that this may well be the most important function of criticism, but judgment, if it is to be of any value, entails more than a license to take pot shots at other people's work. It requires a reasoned consideration of facts and relationship among facts. Otherwise, we might as well declare all scientific scholarship invalid on the basis of the not so self-evident truth that any potential source of unwanted influence necessarily determines the results of inquiry.

The function of criticism that consideration of the previous issues suggests is to determine the extent to which a probability statement stands unencumbered by the constraints of inaccuracy and artifact. Given the assigned meaning of criticism, further discussion of its functions may appear to transcend the limits of its definition. The term *acceptability,* however, may warrant the examination of one more function.

Because of the increasingly important role science has come to play in technologically advanced societies, the problem of ethical considerations is a matter of increasing concern.[8] I realize that this issue might not be greeted enthusiastically by those who hold that science is value free. But that description of science is itself ideological, which in a sense renders it self-contradictory. In any event, it is worthy of examination. The observation of scientific practice surely would not lend credibility to the value free perspective. The very choice of problems and the means to investigate them entails priority judgments, which implicitly suggests that some things are better or more important than others—at least, in the mind of the individual making those choices. From where I stand, this is a matter of value. The issue ultimately reduces to a concern with the kinds of biases that values may impose on inquiry, but as Kaplan points out, "Values make for bias, not when they dictate problems, but when they prejudge solutions."[9] Since the question of values in science is much broader than that of ethical considerations, however, I shall restrict the remainder of my remarks to the more limited case.

Kelman has suggested as an ethical standard research on human behavior ought to meet that a subject should not "leave the laboratory with greater anxiety or lower self-esteem than he came with."[10] Nilsen's argument that "an ethical issue arises whenever the behavior of one person affects the well being of another,"[11] however, provides a broader context in which it is appropriate to pursue such matters. Since the communication scientist often is in a position to

[8] See Jerome Ravetz, *Scientific Knowledge and Its Social Problems* (New York: Oxford University Press, 1971), pp. 389-315.

[9] Kaplan, p. 382.

[10] Herbert C. Kelman, "Human Use of Human Subjects: The Problem of Deception in Social Psychological Experiments," *Psychological Bulletin,* 67 (1967), p. 8.

[11] Thomas Nilsen, *The Ethics of Speech Communication* (Indianapolis: Bobbs-Merrill, 1966), p. 2.

affect the well being of others, the question of the acceptability of information produced under ethically provocative circumstances is one of legitimate concern. I realize that I am using the term *acceptability* in a different sense from its prior usage. Whereas it previously referred to the justifiability of a knowledge claim in relation to a particular set of facts, in the present context, it refers to the justifiability of the conditions under which such a claim is produced.

To show why the ethical function of criticism should be included as an integral part of its role. I again find myself in need of resorting to a hypothetical example. Assume that someone wanted to study the long term effects of smoking in the days before its consequences were fully suspected or appreciated. Were such an individual to decide that an experiment was the best way to pursue the problem, he or she conceivably could have been responsible for creating an incidence of lung cancer or heart disease in the experimental group—albeit unintentionally—that otherwise might not have occurred. Debriefing the subjects would somehow seem to be a rather feeble assumption of ethical responsibility under the circumstances. The example, of course, is extreme. A careful scientist, we assume, would not take such risks with the unknown, but then the studies of venereal disease that were conducted in Tuskeegee, Alabama are a painful reminder to the contrary.

In what ways does the kind of scholarship in which those of us in communication theory and research engage affect the well being of other people? Among other things, we frequently lie to the participants in our investigations. I am not sure that I would accept the proposition that lying to others always adversely affects their well being, but neither would I entertain the proposition that it never has such an effect. The point is that the activity raises an ethical question that itself does not automatically imply the appropriate judgmental response. The role a critic might usefully play is to render a judgment after having examined the fabric of circumstances of which the activity in question is a part.

I am not proposing that critics become moral censors or dictators of the conduct of inquiry in communication theory and research, nor do I believe that one need run the risk of calling professional colleagues unethical by being concerned with these matters. Disapproval and questionability can be expressed without being either libelous or unprofessional. Kelman's response to Milgram's research on obedience well illustrates this argument. In his reaction, he notes that "subjects in these experiments may well have emerged with lowered self-esteem, having to live with the realization that they were willing to yield to destructive authority to the point of inflicting extreme pain on a fellow human being."[12] It seems to me that we do need to be conscious of the possibility that some of what we do may adversely affect the what we do may adversely affect the well being of others, and when an individual scientist appears not to display such consciousness, it is not only the right but, I would argue, the professional responsibility of the critic to respond.

Aside from the deception of participants in scientific studies of communication behavior, there are other matters that call attention to ethical considerations. We take students from classes, we test them, we promise them rewards, we threaten them with punishments, we put them into stressful situations, and some of us use them over and over and over. I recall a new graduate

12 Kelman, "Human Use . . . ," p. 4.

student several years ago who informed me that he was happy to be at Indiana because of the availability of "a new subject pool." It seems as if that to which he had previously had access was "badly contaminated" owing to the fact that students in basic classes spent most of their course time participating in experiments. There is something insidious about that kind of language and the attitude it represents. One, moreover, has to question the propriety of the excessive use of the students involved, not so much because of the contamination factor but because of the possibility that their opportunity to learn was unnecessarily restricted. I cannot say that it was, but that it insufficient to eliminate the underlying ethical concern.

The issues I am raising are by no means new to the dialogue surrounding scientific inquiry.[13] It is beyond the scope of this essay, moreover, to suggest the precise set of standards by which to judge the practice of science in communication theory and research. My purpose is simply to suggest that certain matters which do not directly affect data-probability claims nonetheless raise questions that require a critical judgment. In reflecting on the issue, I think it is more easily possible to begin giving form and substance to the role of criticism.

As the science of communication begins to mature, additional critical functions may be required—especially if it passes from a pre-paradigmatic to a paradigmatic state. The standards of simplicity, elegance, and parsimony that are characteristic of criticism in other disciplines, for example, might begin to acquire utility in the communication critic's repertoire. Until such a condition exists, however, the functions suggested can do much to elevate the current status of criticism.

In addressing the issue of what should be the functions of criticism, I may have inadvertently created the impression that I believe no one is currently executing the two I have identified. But that is not my argument. The point I wish to make is that these functions have not been formalized or recognized as unifying principles of the type that will give our criticism a distinctive character. The difficulty I perceive with the first three functions mentioned is the absence of any kind of consistent intellectual framework within which they are applied. Alone, the functions of screening, generating new research, and synthesizing scholarly inquiry are subject to a certain amount of capriciousness on the part of the individual critic. Although valuable, these types of activities do not provide an especially clear or meaningful role for criticism.

In retrospect, it becomes clear that the five functions discussed in this essay are not independent. The first three one might label *utilitarian* functions and the last two *guiding* functions. The two types must be applied in conjunction if the role of criticism is ever to acquire sharp definition. When criticism is considered from this perspective, I believe that we can go a long way toward eliminating the sort of judgment to which many alleged critical acts reduce; that is, a mere statement of personal preference or taste. Remove the rhetorical embellishments in which such utterances are clothed, and one is left with the following statement as an index of critical sophistication: "I did not like this piece of scholarship." Although this type of judgment may represent an honest expression of opinion, it is merely an affective response that does little to illuminate the weaknesses of a particular inquiry. It is rather more like

[13] For a fruitful discussion of these issues, see Herbert C. Kelman, *A Time to Speak* (San Francisco: Jossey-Bass, 1968), pp. 202-225.

Truman Capote's evaluation of Jack Kerouac's writing; that is, "Jack Kerouac is not a writer, he's a typist."[14] An effective critic's observations may be less witty, but they certainly can be more substantive.

Lest the essay leave the impression that criticism is being assigned a more important role than it justly deserves, Kaplan's caveat may allay that concern: "The situation in science is not unlike that in the arts: the critic with his standards discourages daubers, but he also becomes the mainstay of the Academy, and art eventually passes him by."[15] The warning notwithstanding, criticism can and should have a more meaningful role in communication theory and research than it presently enjoys, and I for one hope that it will.

[14] Truman Capote, on NBC's "Tomorrow," May 16, 1978.

[15] Kaplan, p. 26.

SUGGESTIONS FOR THE DESIGN OF EMPIRICAL COMMUNICATION STUDIES

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A FREQUENTLY experienced revelation by novices in empirical research is the realization that the enterprise is not purely formulary. Rather, creativity and judgment are required at all stages of developing one's design. The centrality of judgment to the research process has been emphasized by Stanley Schacter:

> Like it or not, science is a capricious business. We always must decide if an experiment is an adequate test of a hypothesis, if a particular subject is an adequate preparation, if a particular test measures what we presume it measures, and so on. . . . It's an untidy world but so many people work on similar problems that over time, things do get sorted out. It's a messy way to live but imagine the alternative scientific world without benefit of Mendel, Dalton, or Newton.[1]

Of the manifold approaches to pursuing a given research question, some clearly are better than others. Moreover, some questions are more worthy of pursuit than others. Thus, the role of the critic, which I have been asked to address, becomes one of providing guidelines for the evaluation of research questions and the choice of designs with which to pursue them. Judgments must be made at three general levels. First, the design must have a high degree of internal validity. The study is pointless unless we may reasonably conclude that, at least within the particular context studied, the variables asserted to be related to each other, rather than other intervening variables, are in fact the operative ones. Secondly, the study should afford a high degree of external validity. Hopefully, the results will generalize to a wide range of phenomena beyond the specific context studied. And finally, and certainly most problematic, the research question itself should be of significance to the audience to whom the report is presented.

During the 1950's and early 1960's when few people in speech communication were trained extensively in empirical methodology, our journals contained frequent instances of studies lacking internal validity. For instance, studies attempting to contrast oral and written style typically were confounded with amount of preparation. Often, the written messages were carefully composed whereas the oral ones reflected spontaneous speech, thereby making it nearly impossible to determine whether encoding modality or amount of preparation was primarily responsible for any differences noted in the messages. In recent years, however, instances of internally invalid studies have become less plentiful.

Despite the realization of more tightly controlled studies, I shall argue that this achievement has occurred at the expense of the external generalizability of our

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[1] Stanley Schacter, "Sin and Error in Science," *Contemporary Psychology, 23,* (1978), p. 5.

work, and moreover, that the primacy of method has led us too frequently to ask questions of little significance. I shall contend, furthermore, that such a sacrifice is unnecessary: it is altogether possible to have tightly controlled studies dealing with significant questions within the framework of a design which permits generalization to a wide range of phenomena.

This essay should not be read as a denigration of rigorous methods, for without internal validity, no generalizations are possible. Too often, however, the method rather than the research question seems to be the controlling force in a study. If a researcher is familiar with a particular measuring instrument or a statistical technique, he or she may seek questions amenable to study by such methods rather than searching for techniques to answer substantial questions of interest. Gadlin and Ingle argue a similar point with respect to research efforts in psychology:

> Rather than selecting for research those phenomena suited to our methods, we ought to shape and develop our methods to fit phenomena. This is not to argue that considerations of scientific methodology are insignificant. Indeed, at times methodological developments can be crucial for paradigm change. Method is essential to science, but it can emerge from confrontation with phenomena rather than being applied to them.[2]

The remainder of the essay, then, attempts to offer suggestions for selection of questions of greater significance and for designs which permit richer and broader answers to the questions we pose.

Selection of Research Questions

The upper limit on the potential of any research endeavor is imposed by the significance of the question asked. Brian MacKenzie asserts, "There is no methodological substitute for good ideas, at every stage of research . . . ; and while methodological tools are necessary to comparing and developing these ideas, scientific progress is possible only if the tools, rather than the materials to be worked, have the supportive role."[3] The crucial issue, of course, is how we know when a question is worthy of investigation. Undoubtedly, reasonable people will disagree on this matter. In general, however, it would seem that questions tied to the fundamental concerns of a discipline should have preference. Accordingly, I offer four suggestions for implementing this objective.

Variables Must Have Conceptual Coherence

Research questions cannot yield meaningful results unless the variables addressed are conceptually clear. Although this is a fundamental principle of good design and in no way unique to our discipline, it is violated frequently enough in our journals to warrant mention.

Two criteria seem useful in assessing such coherence. First, the variable itself and the specific levels or conditions to be studied must be defined carefully enough to permit another researcher to replicate the design using different specific empirical embodiments or operational definitions, as such embodiments are sometimes called. Consider for instance, Gundersen and Hopper's report that an "ineffectively composed" message produced lower ratings than an effectively composed one on selected dimensions of credibility and scales measuring general

[2] Howard Gadlin and Grant Ingle, "Through the One-Way Mirror: The Limits of Experimental Self-Reflection," *American Psychologist*, *30* (1975), p. 1007.

[3] Brian D. MacKenzie, *Behaviorism and the Limits of Scientific Method* (Atlantic Highlands, New Jersey: Humanities Press, 1977), p. 155.

effectiveness.[4] The "ineffectively composed" message represented a revision of the effective one in which authority citations were removed, appeals to emotions were eliminated, and the organization was altered to provide less obvious connections among ideas. Since different researchers might well have varying notions concerning what constitutes an emotional appeal or obvious connections among ideas, it would be difficult to replicate the study. If a subsequent study produced conflicting results, the researcher might reasonably conclude that his mutilations had not been severe enough, which in essence, then, could reduce the hypothesis to circularity.

A slightly different version of this problem of conceptual clarity appears in the work of Lashbrook, Snavely, and Sullivan.[5] These investigators studied the amount of information contained in messages and defined high, medium, and low levels of information as sixteen, eight, and four assertions per message. On the surface, the operational definition, if not the conceptual definition, seems straightforward. Upon closer scrutiny, however, the ambiquities of both the operational definition and its underlying concept become apparent. Another researcher could not readily determine how different one statement must be from another to be considered a separate assertion. And more important, the assumption underlying the basic variable of "information quantity" is that there is such a phenomenon as a unit of information with any one unit being captured in a single assertion. Even the carefully reasoned position of Charles Osgood attempting to analyze sentences into discrete assertions attracted few followers, due to the obvious difficulties in defining the boundaries of such units of analysis.[6] Thus, a variable such as amount of information seems fated to produce a confused body of results similar to those in fear appeal research, which suffered from the same failure to define levels of the variable in a way which would permit comparability of results from one study to another.

A second criterion for assessing the coherence of variables concerns whether alternative empirical embodiments will yield different or perhaps even contradictory predictions. Currently, a number of researchers in our field are interested in self disclosure. Such research is likely to produce conflicting results unless more precise conceptualizations are forthcoming. Typically, self disclosure is conceived broadly to include revelation of information about intimate actions or thoughts of the speaker. Research involving this variable generally is predicated on the assumption that self disclosure leads to a positive response from the message recipient. It requires little imagination, however, to think of specific empirical embodiments which might lead to quite negative responses. Offering details of tooth-brushing procedures for example, might reveal the speaker as boring. Stressing one's accomplishments could create the image of an egotist. Similarly, disclosing other desires and actions might produce impressions of the individual as malicious or thoughtless. Thus, it is not surprising that Gilbert reported that the high self discloser was less well liked and thought to have lower self esteem than the low self discloser, when the disclosure occurred in an inappropriate context, in

[4] D. F. Gundersen and Robert Hopper, "Relationships between Speech Delivery and Speech Effectiveness," *Communication Monographs, 43* (1976), 158-165.

[5] William B. Lashbrook, William B. Snavely and Daniel L. Sullivan, "The Effects of Source Credibility and Message Information Quantity on the Attitude Change of Apathetics," *Communication Monographs, 44* (1977), 252-262.

[6] Charles E. Osgood, "On Understanding and Creating Sentences," *American Psychologist, 18* (1963), 735-751.

particular, a brief encounter with a stranger in an academic setting.[7]

Rationale Underlying Research Question Must Be Clear

A clear rationale is essential both to the selection of the research question and to guiding the choices to be made in designing the study, a point to be developed later. What is required is a coherent and plausible argument explaining why a particular prediction should hold or why a relationship is anticipated to exist among a set of variables.

Of particular value are studies which focus on rival but seemingly plausible rationales. One such investigation is the research by Watt and Krull on the effects on children of viewing aggression on television.[8] Since reasonable cases can be made that such viewing will lead to cathartic release, arousal, or facilitation by imitation, an attempt to resolve these discrepant positions is useful.

Attention to the rationale may produce research which captures more major features of the phenomenon under consideration than might occur otherwise. Let me illustrate. A number of people have investigated the question of differences between men and women's language behavior. Some such ventures have been undertaken with little thought concerning what kinds of basic differences might reasonably be expected. Hence, these investigators have simply assessed a variety of easily observed variables (typically linguistic features.) The better research in this area takes as its starting point the kinds of self concepts held by the two sexes. Thus, for instance, it focuses on variables such as qualifiers and modifers which may reflect the less assertive nature of women. With a bit more thought, however, it would seem possible to design studies yielding even yet richer results. One difference frequently noted between the sexes is the greater interpersonal orientation of women. Pursuing this notion, a student in one of my classes last spring designed a study in which men and women were asked what they would say to a friend facing an interpersonal problem (loss of scholarship, dissolved romance, etc.) She discovered that men concentrated primarily on offering solutions to the problem, whereas women combined solutions with attempts to offer comfort and sympathy. In my judgment, this reflects a more fundamental difference in language behavior than the use of linguistic devices such as tag questions or modifiers.

More important still is the possibility that an unclear rationale will produce misleading results. Such seems to me the case in a study by Bradac, Hosman, and Tardy on the reciprocity of disclosure and language intensity.[9] The general hypothesis was that a speaker responding to another will be judged more favorably by an observer if he matches the original speaker in terms of level of self disclosure and language intensity. With minor exceptions the specific predictions were confirmed. I would argue, however, that the rationale was grounded in the wrong concept. Rather than reply "similarly," I would take the position that the second speaker must reply "appropriately" to the complex of constraints involved in a given

[7] Shirley J. Gilbert, "Effects of Unanticipated Self-Discourse on Recipients of Varying Levels of Self-Esteem: A Research Note," *Human Communication Research, 4* (1977), 368-371.

[8] James H. Watt, Jr. and Robert Krull, "An Examination of Three Models of Television Viewing and Aggression," *Human Communication Research, 3* (1977), 99-105.

[9] James J. Bradac, Lawrence A. Hosman, and Charles H. Tardy, "Reciprocal Disclosures and Language Intensity: Attributional Consequences," *Communication Monographs, 45* (1978), 1-17.

situation. Level of intensity and disclosure are only two, and perhaps relatively unimportant ones, of the many constraints operative. Others surely would include those deriving from role relationships and the nature of the situation. For instance, if Eric Hoffer visited the White House, I suspect that Rosalyn Carter would be judged less favorably if she attempted to match Hoffer's level of intensity than if she behaved more in accord with our expectations of a first lady.

A special hazard one faces in developing a rationale for our research questions is that we frequently borrow theoretical positions from other disciplines, since few coherent overarching frameworks have been advanced within our own field. The difficulty with this approach lies in the fact that theories necessarily involve abstractions from reality, and all too frequently we mix and match positions with little regard for the range of phenomena from which they originally derived. Sometimes predictions are advanced which contradict beliefs based on wide experience.

Take, for example, a recent study by Plax, Bodaken, and Sereno.[10] By combining views of Schacter on people in anxiety producing situations, Sherif, Sherif, and Nebergall on ego-involvement, and Steiner on response alternatives in dissonance producing situations, Plax et al. conclude that low-ego-involved individuals will have greater desire than highly-involved individuals to communicate when faced with an anxiety arousing communication. They predict further that the same relationships will obtain as well when the communication is non-anxiety producing. Taken together, the thrust of the hypothesis is that individuals who are not involved will have higher desire to communicate than others upon the receipt of any kind of message. This defies typical human experience. Individuals may be reluctant to communicate about extremely ego-involving matters, but they most likely do not communicate primarily on matters of no personal consequence. Although the authors report support for their position, I would argue that the seeming confirmation was obtained because they measured pleasure in communicating rather than desire to communicate. At times, one may feel the need to communicate despite the fact that it will be unpleasant to do so.

In general, it would seem that to be minimally acceptable, the rationale offered for a research question should not contradict generalizations derived from a broad range of experience. A wise colleague once remarked to me that it is easier to teach graduate than undergraduate students. Graduates require only that our generalizations be consistent with one piece of published research, whereas undergraduates expect propositions advanced to correspond with experiences.[11] The latter seems to be the more sensible as well as the more rigorous guideline for testing our rationales.

Focus on Underlying Processes

Of particular value are research questions which illuminate an underlying process as opposed to dealing with relationships among surface-level variables. By surface-level variables I mean phenomena whose membership is determined by relatively easily observed characteristics, such as sex, race, mode of dress,

[10] Timothy G. Plax, Edward M. Bodaken, and Kenneth K. Sereno, "Anxiety Arousing Messages and Ego-Involvement as Determinants of Communicative Predispositions, *Human Communication Research, 3* (1974), 335-343.

[11] Howard Maclay of the Institute for Communication Research of the University of Illinois, informal conversation.

lexical usage, and so forth as opposed to internal states of individuals or decision processes such as selection of message strategies which must be studied much more indirectly. Credibility, for instance, has been shown to be affected by a wide range of surface level variables. With little effort, an almost unlimited number of surface level variables could be manipulated in such a way as to alter credibility ratings, simply by choosing embodiments of the particular variable which would set the speaker apart as deviating from norms (for instance, unusually wrinkled attire, inclusion of an exceptional number of adverbs, etc.) But such exercises do not expand our understanding of how credibility operates to produce more or less favorable reactions to the message.

Similarly, a host of messages variables have been manipulated in studying attitude change. It does not seem particularly useful to vary numbers of pieces of evidence or extensiveness of citation unless we simultaneously investigate how such alterations affect the thinking of the message recipient to produce differential degrees of attitude change.

By contrast, recent work by Applegate and Delia extending the work of Basil Bernstein emphasizes the utility of moving from a surface-level prediction to a generalization of greater explanatory force. Bernstein argues that the speech of middle class speakers is more elaborated and internally coherent than that of lower class speakers.[12] Most of the research on Bernstein's claims has employed codings of surface level linguistic features such as pronoun usage, number of adjectives, and so forth. By coding appeals and strategies in terms of manifest elaboration of sensitivity to individuality of perspectives, Applegate and Delia demonstrated that the operative explanatory variable is not social class, but the quality of the orientation taken to conceptualize persons.[13] Individuals with more abstract dimensions for conceptualizing persons tend naturally to elaborate messages in ways manifesting sensitivity to individual perspectives. Bernstein's generalization quite likely has some validity in a class-bound culture such as that in England, but his hypothesis is of less explanatory value than the position taken by Applegate and Delia.

I do not take the view that surface-level variables should never be studied. But to advance from surface-level variables to explanation would require either that a synthesizer be familiar with the full range of work with respect to a given phenomenon, or that the synthesizer have more creative insights than the original researcher.

Concentrate on Variables which Reflect the Core of a Conceptual Position

My final suggestion is perhaps the most crucial, and also the most difficult to implement. Specifically, the researcher should strive to pose questions which are central to the fundamental concerns of the discipline. I would argue that fields which make programs through empirical research do so because there is some consensus concerning what processes are of central interest to the discipline. Psychology, for instance, has bodies of literature organized around the processes of perception, memory, learning, attitude

12 Basil Bernstein, *Class, Codes, and Control: Theoretical Studies Towards a Sociology of Language* (London: Routledge and Kegan Paul, 1971).

13 James L. Applegate and Jesse G. Delia, "Person-Centered Speech, Psychological Development, and the Contexts of Language Usage," to appear in *The Social and Psychological Contexts of Language*, eds. Robert St. Clair and Howard Giles (Hillsdale, N.J.: Erlbaum, in press).

change, etc. Speech communication has much less clearly defined phenomena which guide our research activities. Probably the two variables most frequently studied have been credibility and attitude change. Although work in these areas has been of some value, it does little to exploit the unique perspective of those of us trained in speech communication.

More reflection could, however, suggest phenomena which are at the core of our concerns. For example, some of us interested in communication skills in children have taken the position that adaptation to the perspective of others is essential to developing flexibility as a speaker. Thus, we developed a set coding schemes indicating the degree of adaptation in interpersonal and persuasive communications. And this is but one example. My hunch is that we would profit from reflecting on the generalizations which we advance with confidence in our role as teachers to see whether the phenomena we discuss in the classroom might well become the focal point for more research. For instance, I suspect that many members of our field who teach interpersonal communication have some faith in the validity of Jack Gibb's characterization of defensive versus supportive communication.[14] Although many of us may see these two modes of communication as more central to interpersonal communication than a variable such as self disclosure, little of our research reflects such a view.

The goal of evolving clear conceptions of the processes central to communicative effectiveness is, of course, not likely to be realized quickly. Thus, our immediate efforts should focus on phenomena which describe broad ranges of behavior of interest to our field. It seems strange, for example, for Lashbrook and his associates to pursue an understanding of attitude change among apathetics, which at best would be appropriate in cases of messages with forced or accidental exposure, when we know so little about attitude change in general.[15] If, however, we combine an effort to specify the phenomena we wish to understand with the suggestion that we look at explanatory rather than surface-level variables, we can move closer to the objective of understanding the processes at the core of speech communication.

Designing Studies

In two interrelated ways our designs might easily be improved: we need greater attention to the generalizability of our results and to the richness of the answers which we supply to our questions. In a carefully reasoned paper suggesting how increased ecological validity might be achieved, Urie Bronfenbrenner contends that these goals can be met without sacrificing the internal validity of the study:

> To corrupt a contemporary metaphor, we risk being caught between a rock and a *soft* place. The rock is *rigor*, and the soft place *relevance*. . . . The orientation proposed here rejects both the implied dichotomy between rigor and relevance and the assumed incompatibility between the requirements of research in naturalistic situations and the applicability of structured experiments at an early stage in the scientific process.[16]

Below are some guidelines which hopefully will be useful in maintaining rigor while at the same time providing richer and broader answers to our questions.

[14] Jack G. Gibb, "Defensive Communication," *Journal of Communication, 11* (1961), 141-148.

[15] William B. Lashbrook, William B. Snavely, and Daniel L. Sullivan, The Effects of Source Credibility and Message Information Quantity on the Attitude Change of Apathetics," *Communication Monographs, 44* (1977), 252-262.

[16] Urie Bronfenbrenner, "Toward an Experimental Ecology of Human Development," *American Psychologist, 32* (1977), p. 513.

Consider Diverse Approaches to Answering the Question

A novice empirical researcher is more likely to describe what he would like to do than what he would like to know. The research question, should, however, be the focal point at all stages of the research enterprise. Ideally, we should consider widely divergent alternatives to answering the question before selecting a method. Too often, however, this is not the case. A typical course is to read a study which suggests a variable which might interact with those in the original investigation, and simply to replicate the original study with the single alteration being inclusion of the additional variable. Such a process precludes consideration of alternative approaches. Phoebe Ellsworth comments on a similar tendency in psychological research:

> Virgin territory is rare, however, and additional considerations come into play in the secondary growth of follow-up studies. At this stage, the investigator must ask, How has this topic, or phenomenon been studied before? Unfortunately, many investigators regard this question as the equivalent of the question: How does one study the topic? and they model their contexts as closely as possible on those of their predecessors. For some purposes this is advantageous, for others (such as direct replication) even necessary, but for other purposes, it may be disadvantageous. In any case, the decision should not be an automatic one.[17]

The disadvantage of proceeding without consideration of alternative approaches is that we derive less meaningful results than would otherwise be possible. For instance, the study by Bradac et al. mentioned previously focused on the effects of variations in self disclosure and intensity of language on a series of judgments made of the speaker.[18] One of these judgments was the degree of perceived internality or externality of the speaker. If the authors were genuinely concerned with this variable, they might well have begun with a different manipulation altogether, for I would guess that variations in accepting responsibility for action, for instance, would account for greater differences in judgments of internality.

Similarly, the investigation of factors influencing the desire to communicate, discussed above, might well have been approached in an entirely different fashion.[19] I would suspect that the desire to communicate would be contingent upon the nature of the situation and the role relationship with the other interactant, perhaps to a greater degree than on level of ego involvement in the topic under consideration. Consequently, if I wanted to know what variables are most relevant to desire to communicate, I would provide subjects with a variety of communicative contexts (ranging from informal ones where entertainment or maintaining personal relationships might be primary goals to more formal ones where problem solving might be the focus of interest) and a diversity of role relationships with the other interactant. I would then have each subject rank order a set of possible topics according to his or her desire to talk about them in the given context to a particular other, and ask the subject to provide a rationale for his selection. In this way, I could begin to determine the dominant factors in the desire to communicate, while simultaneously controlling the individual differences in the general level of desire to communicate by using a ranking procedure rather than scaled responses.

Considering diverse approaches not only enhances the likelihood that the

[17] Phoebe C. Ellsworth, "From Abstract Ideas to Concrete Instances: Some Guidelines for Choosing Natural Research Settings," *American Psychologist, 32* (1977), p. 605.

[18] Bradac, Hosman, and Tardy.

[19] Plax, Bodaken, and Sereno.

design chosen will yield rich results, but also increases the probability of use of a series of studies testing different empirical predictions emanating from the same general hypothesis. The combination of results of multiple predictions embodied in the same hypothesis provides a rigorous test of the hypothesis, since the potential of systematic influence from intervening variables is diminished.

One of the most persuasive bodies of research I have read is that of Schacter investigating the general hypothesis that obese individuals eat in response to external cues, whereas indivduals of normal weight respond to internal cues.[20] In one set of studies he observed the phenomenon as it occurs naturally. He found that overweight individuals are more likely than normal weight people to abandon meal contracts in dormitories (the assumption being that institutional food is less palatable than that from alternative sources). Overweight pilots arriving in a new time zone tend to eat at meal time in the new location rather than at a reasonable interval since the previous meal. And overweight people were found to be more likely than normal weight ones to fast on religious holidays. Schacter's work also included controlled studies in which relevant external cues were manipulated. For instance, altering a person's perception of time by manipulating the face of a clock affected the consumption of overweight individuals but not persons of normal weight. Diminishing the tastiness of the food offered reduced the amount which an overweight person ate whereas the normal weight person ate his or her usual amount. Most compelling, however, was Schacter's attempt to demonstrate directly that the overweight person cannot accurately read his or her own internal state, reflected in a study in which he assessed the response to balloons inflated in the stomach. While any one of these studies might be criticized, the combined force of this line of work is considerable. The point to be made here is that a research using alternative approaches produces more compelling evidence for a position by replacing the typical replication procedure with one in which a diverse set of testable implications is derived from the same hypothesis.

[20] Stanley Schacter, *Obese Humans and Rats* (New York: Halsted Press, Division of Wiley, 1974).

Variables Require Realistic Embodiment

The empirical embodiment or operationalization of variables might be achieved more realistically in many cases. At this point I am concerned primarily with the independent variable. In attempting to capture the very core of the concept in which we are interested, the most fundamental choice we face is whether to employ instances of the variable as they occur naturally or to manipulate them. If one reads our journals, however, this choice might escape notice, for in most studies it is simply assumed that the variable will be manipulated. We need to recognize that this approach may affect our findings. Bronfenbrenner's work in child development, for instance, documents systematic differences in the behavior of children in natural and laboratory settings.[21] Since few comparisons are possible between studies in our field involving natural observation and manipulation, we cannot be sure what the nature of the differences is, but we should recognize that differences likely do exist. To give but one example of an area where results may reflect the method, consider our work in attitude

[21] Bronfenbrenner, p. 516.

change. Typically, we use brief messages in settings in which at least minimal attention is appropriate. In a natural setting, the same message might well be completely ignored by the same individuals.

Obviously, the use of naturally occurring instances of some variables is more easily accomplished than others. In fact, some studies almost invite the use of natural embodiments of the variable. Recently, Bradley studied the way in which power and status affect the content of statements in a group discussion by manipulating these variables through alteration of credentials of one of the participants.[22] Different results might have been obtained in a situation in which participants were more vitally involved. Bradley reports, for instance, that more friendly and reasonable statements were directed toward the high power figure. This seems predictable enough in a situation of low involvement, but had a promotion been under consideration by a faculty committee, or the site of a multi-million dollar project been the topic of discussion by congressmen, my guess is that a high-power figure frustrating the wishes of other participants might have elicited somewhat different responses.

Our preference for use of manipulation over natural occurrences is understandable in our desire to control for contaminating factors. Two counterpositions deserve consideration, however. First, we may generate a series of generalizations which have scant relationship to real phenomena. Bronfenbrenner argues this point:

> Human environments and—even more so—the capacities of human beings to adapt and restructure these environments are so complex in their basic organization that they are not likely to be captured let alone comprehended, through the simplistic unidimensional research methods that make no provision for assessing ecological structure and variation.[23]

Second, it is possible to randomize the effects of potentially contaminating variables by using multiple embodiments of the variable. For instance, in the study by Bradley previously alluded to, had I chosen to study naturally occurring variations in power and status, I would have done so in multiple instances to ensure that some unique feature of a given circumstance was not responsible for whatever differences were observed. And if our predictions cannot be confirmed across diverse cases, then we should recognize that the prediction itself requires modification. To carry the example further, the hypothesis in the Bradley study might be specified as limited to topics of low involvement and to behavior toward a power or status figure who does not thwart the basic will of the group.

Use of naturally occurring instances of variables seems of special importance in research questions attempting to assess absolute values with respect to some dependent variable rather than studying interrelationships among variables. For example, if we want to know the average utterance length of three-year-olds, we might do well to observe them in natural settings. But if our concern is with whether familiarity of message recipient affects the utterance length of three-year-olds, we might just as reliably use controlled manipulations. In the latter case, the absolute length of utterance may differ from those observed to occur naturally, but the relative impact of differences in the message recipient may still be observable.

Certainly, I am not advocating aban-

[22] Patricia Hayes Bradley, "Power, Status, and Upward Communication in Small Decision-Making Groups," *Communication Monographs*, 45 (1978), 33-43.

[23] Bronfenbrenner, p. 518.

donment of our traditional methods of manipulating variables. I expect that they will rightfully continue to play a prominent role in our research. With greater reflection, however, it would seem possible to devise manipulations which are more realistic embodiments of the variables under consideration. Otherwise, our research efforts may prove pointless. For instance, the early work in the effects of hesitation phenomena on credibility ratings seems of limited utility due to the atypicality of the manipulations. Rather than inserting hesitations within the stream of language at junctures where they would occur normally, researchers inserted them after every nth word.[24] It was not surprising, then, that the results indicated lower ratings on some dimensions of credibility, for such random insertation must have made it difficult to follow the sense of the message, whereas hesitations at normal junctures might actually have facilitated comprehension and, thus, have had a positive effect on speaker's image.

We should at least be fully aware of the extent to which our manipulations may well be a controlling factor with respect to our results. A recent study compared the effects of variations of self interest and desire for liking on the selection of persuasive strategies using two different methods.[25] Subjects provided with a list of persuasive strategies from which to choose selected better adapted strategies than subjects who actually constructed messages. Moreover, the effects of the independent variables observed with real messages disappeared with the selection procedure, since the overall quality of all strategies was better in the latter case. I would contend that the results obtained with actual messages are more likely to reflect the way in which the phenomena operate within real situations, primarily because individuals normally engage in actual message composition but seldom are asked to reflect on what the underlying structure of these messages might be like. To draw an analogy, if one wanted to know how good someone's grammatical usage is, it would probably be more valid to evaluate an actual language sample obtained for an individual than to judge his or her diagramming of sentences. In general, the point to be emphasized is that we should consider carefully the way in which we manipulate our variables to ensure that the task is reflective of the variable with which we are concerned. This should not be taken to mean surface-level similarity. Asking a student to participate in a group discussion may seem representative of actual behavior. But if the intellectual process of such an exercise does not reflect the one in which he or she typically engages, we still do not have a realistic embodiment of the variable.

One special class of variables deserves mention at this point. In particular, when characteristics of subjects are treated as independent variables, as in the cases of race, sex, or social class, we need be attentive to the degree to which the subjects are typical of the group they represent. For instance, research sex differences in language may not reveal differences in a sample of college students of the magnitude that might be found in a broader sample. In general, I am not particularly concerned with the common criticism concerning the limitation of our subjects to college students unless there is reason to suspect that

[24] Kenneth K. Sereno and Gary J. Hawkins, "The Effects of Variations in Speakers' Nonfluency upon Audience Ratings of Attitude toward the Speech Topic and Speakers' Credibility," *Speech Monographs, 34* (1967), 58-64.

[25] Ruth Anne Clark, "The Impact of Self Interest and Desire for Liking on the Selection of Communicative Strategies," *Communication Monographs*, forthcoming.

some unique characteristic of the subjects interacts with the phenomena in question. For if credibility affects immediate attitude change among college students, I suspect that it does for others as well, despite the fact that the particular figure a college student may find credible may differ from those considered credible by other individuals. But when the independent variable involves demographic subject characteristics, we must exercise special caution to ensure that our subjects are typical representatives of their class.

An overarching concern in the selection of the empirical realization of our variables, whether we opt for naturally occurring instances or manipulation, should be the rationale underlying the research question. For quite different approaches might be taken depending on our rationale. For instance, a number of studies with children indicate that persuasive messages are shorter when the recipient is a familiar rather than unfamiliar person.[26] Two alternative accounts are offered for this finding: the first is that the familiar other (mother, for example) may be expected to feel an obligation to comply, and therefore need little justification for action and, second, that it is easier for the child to predict the response of a familiar other; thus, the child uses what he or she considers to be the single best approach rather than multiple approaches. If I were interested in replicating the observed finding, the rationale to which I subscribed would guide my manipulation. If I believed in the explanation involving obligation, I might use as a target a relative with whom the child was not familiar but who might reasonably be expected to have some sense of obligation to the child. If I thought predictability to be the operative factor, I might identify as the message recipient someone who was familiar but had little obligation to the child, such as a neighbor.

[26] The following studies illustrate this point: Ruth Anne Clark and Jesse G. Delia, "The Development of Functional Persuasive Skills in Childhood and Early Adolescence," *Child Development, 47* (1976), 1008-1014; Gorden E. Finley and Carolyn A. Humphreys, "Naive Psychology and the Development of Persuasive Appeals in Girls," *Canadian Journal of Behavioral Science, 6* (1974), 75-80; and James R. Wood, Eugene A. Weinstein, and Ronald Parker, "Children's Interpersonal Tactics," *Sociological Inquiry, 37* (1967), 129-138.

Study Research Questions Under Diverse Conditions

In our quest for internal validity, we typically attempt to hold constant as many conditions as possible. If we are studying the effects of a credibility manipulation on attitude change, for instance, we have a single experimenter administer the same persuasive message consistently in one encoding modality (oral or written) to a group of homogeneous subjects. I am suggesting that a preferable procedure would be to use a wide diversity of contextual factors in which to embed our observations. In the foregoing example, I would prefer that multiple experimenters adminster a variety of persuasive messages (with a correspondingly appropriate credibility manipulation for each), varying the encoding modality for different subjects, and seeking responses from a variety of populations.

This procedure obviously embodies a high degree of risk. For if any of these contextual factors has more impact on the dependent variable than the independent variable, then the effects of the independent variable will be obscured. But I would argue that this is a risk that we should be willing to take. The alternative is to phrase our research questions in a highly restricted way; that is, to specify the relevant contextual features which were held constant. And

even if we limit our generalizations, we may be unaware of which features within the situation actually are relevant. The approach I advocate is one less likely to yield significant results. But when significant results are obtained, we have confidence that they reflect a broad range of phenomena, rather than being unique to the peculiar combination of features embedded in the specific instance observed.

This call for diverse conditions applies not only to the contextual features of the situation in which the variable is embedded, but to the actual embodiment or operationalization of the variables themselves. Thus, just as I argued for the utility of multiple testable hypotheses, I also favor the use of multiple manipulations or natural instances of a variable within a single study. In my course in theories of persuasion, for instance, thirteen student researchers gathered data to answer the same research question, with each researcher providing his or her own embodiment of the independent variable. The study grew from work by Flavil Yeakley indicating that three perceived roles of the persuader, friend, teacher, and salesman, are differentially effective in converting individuals to a cause.[27] Some students selected existing messages which seemed to reflect these roles. Others constructed messages to fit the categories, while the remainder solicited messages from others based on a description of the role relationship. The degree of consistency across these diverse embodiments of the variable showing superiority for the role of friend provided evidence for the stability of the relationship.

Scattered examples exist within our field of carefully done studies using diverse methods of realizing the variables. Taylor studied the development in early childhood of the ability to manage reciprically coordinated and coherent conversations through observation of children in natural settings as well as through analysis of responses to structured tasks.[28] Similarly, Applegate used diverse approaches in the investigation of the relationship between the abstractness of the system of interpersonal constructs employed in person perception and the use of person-centered communicative appeals in regulative and interpersonal situations.[29] His subjects included children, college students, teachers, and parents, and his methods encompassed structured interviews and naturalistic observations. By combining these approaches, he was able to generalize not only about the relationship between the underlying cognitive abilities and their reflection in communication strategies, but also about the way in which contextual factors mediate the ability to use these strategies.

Select a Form of Observation which Provides Rich Data

Just as researchers in our fields tend to assume that variables will be manipulated rather than observed as they occur naturally, an almost equally strong predilection is for selection of a method of observation which is easily scored and highly reliable, which typically produces responses that are already in scaled form or easily reductible to this form. While such an approach has appeal in terms of the reliability of the data and, more importantly, the ease with which it can be analyzed, the

[27] Flavil Ray Yeakley, "Persuasion in Religious Conversion" Diss. Universiy of Illinois 1976.

[28] Stephen Alvin Taylor "The Acquisition of the Rules of Conversation: A Structural-Developmental Perspective and Methodological Comparison," Diss. University of Illinois 1977.

[29] James Lee Applegate, "Four Investigations of the Relationship between Social Cognitive Development and Person-Centered Regulative and Interpersonal Communication" Diss. University of Illinois 1978.

sacrifices in many instances are great. Frequently, it may be possible to obtain data in a form which would yield greater insights concerning the questions we pose. In general, I would argue for response forms which allow subjects freedom to reveal their thinking with respect to the task with which they are confronted.

The most fundamental reason for employment of such data collection methods is that in some cases it will yield different, and I would contend more valid, answers to our research questions. Consider again the study on selection of persuasive strategies cited above.[30] The methods differed both in the degree of realism of the task and the richness of responses. When subjects were provided with a list of alternative strategies from which to choose, the independent variables of self interest and desire to be liked had little effect on the strategies chosen, for in all conditions subjects tended to choose approaches adapted to the views of the persuadee. In contrast, when subjects actually construced messages, the variable of high self interest, for example, elicited such strategies as threats and explicit statements of blame for the problem. A high desire to be liked led subjects to construct messages typically including such approaches as positive altercasting, emphasis on a sharing of responsibility, and explicit external attributions for the cause of the problem. As argued above, it seems plausible that these differences obtained with actual message composition more likely reflect the phenomena as they typically occur than do the responses yielded by the method of asking subjects to select among alternative strategies.

A second reason for preferring methods giving subjects freedom in responding is the greater possibility of serendipitous findings. If the researcher provides subjects with a list of seventeen persuasive strategies from which to choose, he or she obviously discovers no strategies beyond the original seventeen. On the other hand, if subjects compose messages, they may reveal differential use of strategies not originally considered. To extend the preceding example, I found that the degree to which a message focused on the problem versus the solution was a function of the desire by the communicator to be liked. Thus, in future investigations I would study this variable systematically.

A final major advantage deriving from richer data collection methods is that they may go beyond testing the prediction to yield insight into the validity of the underlying rationale as well. Consider an illustration from research on person perception. Mayo and Crockett predicted that highly complex people would exhibit a recency effect when confronted with information of conflicting valence (that is, either initially favorable information followed by unfavorable information about another or vice versa), whereas low complexity individuals would display a primacy effect.[31] They reasoned that the greater flexibility of the highly complex persons would enable them to integrate the later information, whereas the people low in complexity would essentially disregard information presented after an initial impression had been formed. The primary measuring instrument was an adjective check list, which permitted assessment of the general valence and attributes ascribed to the target person. While the prediction was confirmed, it was not possible to determine whether

[30] Clark.

[31] Clara W. Mayo and Walter H. Crockett, "Cognitive Complexity and Primacy-Recency Effects in Impression Formation," *Journal of Abnormal and Social Psychology, 68* (1964), 335-338.

in fact the original reasoning had been correct. Subjects were asked to write detailed impressions of the individual, but the researchers did not analyze these responses which would have permitted direct assessment of the degree to which the two sets of information had actually been integrated. In subsequent work, Crockett and his co-workers supported the original rationale precisely through a structured analysis of freely reported impressions.[32]

Obviously, the use of methods which provide data in a form which must be then coded into an analyzable form increases the effort that the researcher must expend in the analysis stage. But it seems particularly profitable in terms of the goal advanced earlier of attempting to devise ways of investigating the processes we consider most central to the concerns of our discipline. Two intermediate steps are easy to utilize and could add considerably to the insight offered by our results. First, we could structure responses to go beyond the research question to assessing the underlying rationale as well. One of the best studies in the area of the effect of distraction on attitude change is that by Osterhouse and Brock who suspected that distraction may enhance the effectiveness of a persuasive message because it diminishes the ability of the message recipient to counter argue.[33] In addition to measuring attitude change, they asked subjects after receipt of the message to list all the counter arguments they could. A simple count of these arguments provided confirmation for their line of reasoning.

And a second intermediate step is to ask the subjects to supply, in addition to their easily coded responses, a short justification for those responses. At times, these supplementary data may not be systematically coded, but simply used by the researcher to suggest reasons for the results obtained. At other times, these reasons may be used to judge the original response itself. For instance, a confused body of literature exists on the development of empathy in children, in part, I would contend, because researchers have failed to ask children why they respond as they do. One common task confronts the subject with a series of pictures depicting a child whose facial expression suggests an emotion discrepant with one's expectations of the reactions of a normal child in that situation. A typical picture might show a child smiling while getting a shot from a nurse. If one assesses the ability of the subject to understand the feelings of others solely by noting the emotion attributed to the figure in the picture, the researcher may inaccurately judge such ability. Alternative attributions might be equally plausible. One subject might conclude that the child is happy because he thinks the child soon will leave the doctor's office and receive a promoted treat. Another subject might infer that the child is unhappy but has learned to be brave and not reveal his feelings. Hence the researcher needs some knowledge of the thought process responsible for attribution of an emotion.

[32] See, for example, L. H. Nidorf and Walter H. Crockett, "Cognitive Complexity and the Organization of Impressions of Others," *Journal of Social Psychology, 66* (1965), 165-69; Nels Klyver, Allen N. Press, and Walter H. Crockett, "Cognitive Complexity and the Sequential Integration of Inconsistent Information," a paper presented at the Eastern Psychological Association Convention, 1972; and, Allen N. Press, Walter H. Crockett, and Jesse G. Delia, "Effects of Cognitive Complexity and of Perceiver's Set upon the Organization of Impressions," *Journal of Personality and Social Psychology, 32* (1975), 865-72.

[33] Robert A. Osterhouse and Timothy C. Brock, "Distraction Increases Yielding to Propaganda by Inhibiting Counterarguing," *Journal of Personality and Social Psychology, 15* (1970), 344-358.

Conclusion

It should be obvious that the suggestions in this essay reflect personal preferences and clearly are subject to challenge. But from time to time, we need to examine the assumptions implicit in the methods which guide our work. Olson and Clark make the point well:

> Scientists are captives of their methods. Their theories are only as meaningful and satisfying as their procedures for collecting and analyzing data allow. Methods are the ground rules for scientific inquiry and the definition of its limits. . . . As a consequence, a survey of the methods of a field is in part a survey of its conceptual foundations.[34]

It is, therefore, perhaps less important that researchers in our field concur about the relative value of alternative methods than that we explicitly understand the assumptions implicit within these methods and their implications for our conceptual positions.

[34] Gary M. Olson and Herbert H. Clark, "Research Methods in Psycholinguistics," in *Handbook of Perception, Vol. 7: Language and Speech*, ed. Edward C. Carterette and Morton P. Friedman (New York: Academic Press, 1976), pp. 25-26.

THE IMPACT OF VALUE AND SELF-ESTEEM MESSAGES IN PERSUASION

Bonnie Spillman

FOR years social scientists have been attempting to discover a persuasive strategy that is both effective and long lasting. Rokeach[1] reported a persuasive method which proved capable of producing value, attitudinal and behavioral changes after a five minute message and approximately twenty minutes of laboratory time. Not only were results quick, but persistent; changes persisted up to a year and a half after the experimental session. Success of this and similar experiments was grounded on two operational assumptions. First, persuasion is more deeply internalized and persistent if an individual knowingly changes his/her cognitions and behavior. Second, since values are assumed to be the most central and most connected cognitive elements, changes in values produce more cognitive, affective and behavioral changes than changes in less central and interconnected elements. While these two assumptions seem justified by earlier work, a more careful examination of Rokeach's persuasive messages suggests that the individual's or audience's self-esteem had been challenged simultaneously with the appeal for changing values. This seems to hint that individuals change their values knowingly not because the value change has been internalized, but as a defensive strategy to prevent them from appearing stupid or immoral. If true, then an individual's overall level of self-esteem as well as an immediate threat to self-esteem may be important factors mediating Rokeach's paradigm and may suggest a more plausible explanation for his results. The purpose of this paper is not to dispute Rokeach's findings but to discover whether self-esteem, specifically persuasive messages which negatively evaluate an individual and thus challenge his/her self-evaluation, mediate Rokeach's earlier persuasion experiments and to extend his findings by investigating the role of self-esteem both at an overall personality level and as a message strategy.

The link between values, attitudes, and behavior has been established by Rokeach and subsequent researchers.[2] Rokeach focused on the inconsistency between the values for freedom and equality. After being told that these values were ranked far enough apart on the terminal value scale so as to demonstrate an inconsistency, Rokeach achieved a state of dissonance within his participants; later values showed closer rankings and therefore more consistency. At the same time, participants demonstrated marked changes in both their attitudes toward blacks, the major focus of the persuasive message, and toward

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[1] Milton Rokeach, "Long Range Experimental Modification of Values, Attitudes, and Behavior," *American Psychologist*, 6 (1971), p. 453-549.

[2] Milton Rokeach, "A Theory of Organization and Change Within Value-Attitude Systems," *Journal of Social Issues*, 24 (1968), p. 13-33.

participating in civil rights demonstrations. Other studies have demonstrated the interconnection between values for freedom and equality and ideological or political philosophy.[3] Shotland and Berger[4] have demonstrated a link between the value for honesty and the behavior of returning a pencil after an experiment.

The obvious question from this research revolves around the motivation for change following the induction of dissonance. Rokeach maintains that such changes resulted from simple awareness of inconsistency. Steel[5] indicated that greater changes in behavior were produced in his study by challenging a person's self-esteem than by simply pointing out the individual's inconsistency. After calling respondents to see if they would volunteer time and can goods for community aid, he challenged those who refused by pointing out their laziness and lack of civic-mindedness. Three days later, these respondents volunteered more time and can goods than had been requested.

While these findings and others shed an interesting light on an individual's motivation for maintaining a positive self-image even to an unknown telephone caller, certain laboratory experiments have suggested that a person's overall level of self-esteem may intervene to affect the interpretation and results of such challenges. Dissonance researchers[6] have suggested that challenges may be more threatening and may produce more change for people with high self-esteem than for those with low. The rationale is that a person who already thinks badly of himself will agree with or remain unmoved by a negative evaluation which is consistent with his/her self-assessment. A person with high self-esteem, on the other hand, will need to protect him/herself by proving that such negative evaluations from others are unjustified. These people are highly threatened by evaluations that imply their immorality or stupidity.[7]

Hypotheses

Several hypotheses were advanced based on these empirical and theoretical assumptions.

H1: Individuals exposed to a persuasive message emphasizing both value inconsistency and a threat to self-esteem will exhibit more value, attitudinal, and behavioral change than individuals exposed to a message emphasizing only value inconsistency or a threat to self-esteem.

H2: Individuals with high self-esteem should exhibit more value, attitudinal, and behavioral change when exposed to a persuasive message emphasizing value inconsistency and/or a threat to self-esteem than individuals with low self-esteem.

H3: Value, attitudinal, and behavioral change following a persuasive message emphasizing value inconsistency and/or a threat to self-esteem should persist over time.

METHOD

Subjects

Two hundred eighty-one undergrad-

[3] George F. Bishop, Andrew Barclay, and Milton Rokeach, "Presidential Preferences and Freedom-Equality Value Patterns from the '68 American Campaign," *Journal of Social Psychology*, 88 (1972), p. 207-212.

[4] R. Lance Shotland and Wallace Berger, "Behavioral Validation of Several Values of the Rokeach Scale as an Index of Honesty," *Journal of Applied Psychology*, 54 (1970), p. 433-435.

[5] C. Steele, unpublished paper, University of Utah, 1972.

[6] D. C. Glass and J. D. Wood, "Self-Esteem, Aggression, and Cognitive Dissonance," in P. G. Zimbardo (Ed.) *Cognitive Control of Motivation* (Chicago: Scott Foresman, 1968); J. Gollob and J. Dittes, "Effects of Manipulated Self-Esteem on Persuasibility Depending on Threat and Complexity of Communication," *Journal of Abnormal Psychology*, 2 (1965), p. 195-201.

[7] M. Pilisuk, "Depth, Centrality, and Tolerance in Cognitive Consistency," in R. P. Abelson, E. Aronson, W. J. McGuire, T. Newcomb, M. J. Rosenberg, and P. Tannenbaum (Eds.), *Theories of Cognitive Consistency: A Source Book* (Chicago: Rand McNally, 1968).

uate students enrolled in communication courses participated in the initial testing. Seventy-four students qualified for the experiment by demonstrating a discrepancy in their rankings for freedom and equality. Students were randomly assigned to the three treatment groups and a control group. Of these, 38 students had high, and 36 had low self-esteem. Students voluntarily participated.

Measurement

Value was operationalized through Rokeach's terminal value scale. Individuals ranked eighteen terminal values on a scale of personal importance. They were labeled inconsistent if they ranked freedom in the top one-third and equality in the bottom half of the scale.

Attitude was operationalized by Remmers[8] attitude scale toward social groups with blacks as the specified group. Attitude was defined as the median score of an individual's endorsed statements. Six represented a neutral attitude, and scores above six represented positive attitudes while a score below six represented a negative attitude toward blacks.

Self-esteem was measured by the Tennessee Self-Concept Scale. Self-esteem was represented by the total positive score of the counseling subscale. The positive score reflects (1) this is what I am, (2) this is what I feel about myself, and (3) this is what I do. High and low self-esteem was broken at the fiftieth percentile.

A behavioral measure was adapted from Rokeach's earlier work. A letter, unconnected with the experiment, was mailed to participants requesting them to join an organization whose goal was to improve minority-majority relations on campus. They indicated their degree of behavioral commitment by checking one or more of five activities they would be willing to contribute to the organization.

Procedure

As part of the initial pre-test to determine eligibility, students filled out Rokeach's terminal and instrumental value scales, the Tennessee Self-Concept Scale, and Remmer's attitude scale. While 74 qualified, 64 students participated in the experiment one to two weeks following testing. The students in the inconsistent value and the inconsistent value and negative self-esteem message conditions heard the relevant persuasive message and then filled out an immediate posttest including the terminal value and attitude scales. The students in the threat to self-esteem message condition heard a different persuasive message and then filled out an immediate posttest including the value and attitude scales. Six weeks after the experiment, 55 students filled out the second posttest which was identical to the first. Twenty-eight had high and 27 had low self-esteem. Control group students filled out the pretest and later posttests without hearing a persuasive message. The behavioral measure was mailed to participants two days after the second posttest and was unconnected to earlier laboratory procedures.

The persuasion segment of the experiment consisted of approximately 15 minutes of laboratory time. Messages approximately five minutes in length were written, memorized, and delivered live by the experimenter. Participants then filled out the posttests.

In the message emphasizing inconsistent values, the speaker first noted certain similarities and discrepancies in

[8] H. Remmers, Manual for the Purdue Master-Attitudinal Scale (Lafayette, Indiana: Purdue Research Foundation, 1960).

the Utah and bogus national rankings. Utah rankings were obtained from a pervious pilot study. Similar rankings for freedom and national security were noted followed by the discrepancies between equality and salvation. Noting the discrepancies in ranking for equality (Utah had ranked it low while the bogus national sample had ranked it high and nearly equal to the value for freedom), the speaker asserted that the Utah residents appeared to be inconsistent in their rankings for freedom and equality. No implications of this inconsistency were drawn. This message most closely approximates Rokeach's persuasive strategy.

In the message emphasizing a threat to self-esteem, the speaker made vague references to values but did not refer explicity to freedom and equality or exhibit the Utah-national value ranking chart. The message consisted of conclusionary statements that students in this study exhibited a lack of concern for members of minority groups. Again without reference to any specific values, the speaker asserted that the students appeared to be bigoted and negative toward others less fortunate than they.

The message emphasizing both inconsistent values and a threat to self-esteem combined the appeals from the two previous messages. The same similar and discrepant values were referred to on the chart, and the conclusions and interpretations concerning the students' lack of concern and bigotry were linked directly to the freedom-equality discrepancy.

Results

A 3x3x2 analysis of variance for repeated measures was calculated for four criterion variables: (1) freedom rankings, (2) equality rankings, (3) freedom-equality discrepancy, and (4) attitude scores. The factors were three persuasive treatments, two levels of self-esteem, and three repeated measurements. Winer's unweighted means analysis was used to minimize the effect of unequal cell sizes. Neumann Keuls comparisons were calculated when F scores indicated statistical significance. Since only two letters were returned, no statistics were calculated for the behavioral criterion. Comparisons between treatment and control groups were calculated with Winer's C statistic. Cunnett's T was computed when F scores were statistically significant.

Hypothesis One

Hypothesis one predicted that persons exposed to a persuasive message emphasizing both value inconsistency and a threat to self-esteem would change their values, attitudes, and behavior more than those exposed to a message emphasizing either of these appeals alone. Results did not support this hypothesis. The negative self-esteem condition produced more value change than any of the other conditions (see Table 1). Although some changes in freedom ranking occurred across time, most of the increased consistency between freedom and equality was due to statistically significant increases in the rankings for equality (see Table 1). The message's main effect for equality rankings was significant ($F = 9.697$, $df = 2$, $p < .01$).

The interaction between message and time was also statistically significant ($F = 8.75$, $df = 5, 75$, $p < .01$). Neumann-Keuls comparisons indicated that equality rankings were higher in the negative self-esteem condition than for the inconsistent value condition on the second posttest ($p < .01$). They were also higher for the negative self-esteem condition than the inconsistent value and negative self-esteem condition on the second post-

test ($p < .01$, see Table 1). Analysis of discrepancy scores demonstrated a similar finding. The main message effect for these scores was statistically significant ($F = 5.75$, $df = 2,38$, $p < .01$). The interaction between message and time was also significant ($F = 5.48$, $df = 5,75$, $p < .01$). Neumann-Kuels comparisons on the second posttest indicated that the discrepancy was smaller in the negative self-esteem than the inconsistent value condition ($p < .05$). It was also smaller in the negative self-esteem than the inconsistent value and negative self-esteem condition ($p < .05$, see Table 2).

TABLE 1

MEAN COMPARISONS OF EQUALITY RANKINGS OVER MESSAGE X TIME

	Time 1	Time 2	Time 3
Inconsistent value	14.05	9.99	10.59
Negative self-esteem	12.95	8.56	5.9
Both	13.52	9.06	10.96

TABLE 2

MEAN COMPARISONS OF DISCREPANCY SCORES OVER MESSAGE X TIME

	Time 1	Time 2	Time 3
Inconsistent value	10.3	7.4	6.64
Negative self-esteem	9.6	6.35	2.9
Both	9.2	6.46	6.47

Although the value change was greatest for individuals exposed to the message emphasizing a threat to self-esteem alone, value modification occurred in the other two message conditions. Individuals raised their equality rankings in the inconsistent value condition between the pretest and the first posttest ($p < .01$). Rankings remained higher on the second posttest than on the pretest ($p < .01$). Similar findings were noted for the change in equality rankings in the inconsistent value and negative self-esteem condition ($p < .01$, $p < .05$, see Table 2). A slightly different pattern was found in analyzing the discrepancy scores. In the inconsistent value condition, discrepancy was smaller on the first posttest than the pretest ($p < .05$) and smaller on the second posttest than the pretest ($p < .05$). Changes in the inconsistent value and negative self-esteem condition across time, however, were not statistically significant (see Table 2).

Hypothesis Two

Hypothesis two predicted that subjects with high self-esteem would modify their values, attitudes and behavior more than subjects with low self-esteem. This prediction was not supported. No main or interaction effects involving overall level of self-esteem were statistically significant.

Hypothesis Three

Hypothesis three predicted that value and attitudinal changes would persist between the first and second posttests. The main effect for time was statistically significant for equality rankings ($F = 87.9$, $df = 2,75$, $p < .01$, see Table 1). Equality rankings were significantly higher on the second posttest than the pretest ($p < .01$). As reported earlier, the discrepancy between freedom and equality remained stable between the posttests in the inconsistent value and the inconsistent value and negative self-esteem conditions. The discrepancy diminishes throughout the experiment in the negative self-esteem condition.

DISCUSSION

Although hypothesis two was not supported and hypothesis one was only partially supported, findings from this study shed some light on the motivation and extent of change in the Rokeach paradigm. These findings supported Rokeach's assumption that value modification persists once it occurs. They also

supported the notion that if value modification occurs as a result of threatened self-esteem, the effect will not only stabilize over time, but increase in a positive direction.

Hypothesis One

The results partially support hypothesis one, since value modification occurred with awareness of inconsistency. However, they negate Rokeach's assumption that the cause of such modification is an awareness of the inconsistency between freedom and equality. Threatened self-esteem produced a much stronger motivation than the freedom-equality discrepancy alone and threatened self-esteem was even stronger when completely separated from reference to the specific value inconsistency.

The result noted above leads to the question of why the two persuasive messages containing threats to self-esteem did not produce equal magnitudes of value modification. Gollob and Dittes[9] offer a possible explanation. They found that attitude change was partly a function of the ambiguity or clarity of the threat to an individual's self-evaluation. The clearer the threat, the greater the attitude change. In the persuasive message emphasizing both value inconsistency and a threat, the reference to self-esteem may have been diluted or made less clear by the other presumably relevant information contained in the message.

A disappointing finding is that value modification was not followed by attitudinal and behavioral changes. Two possible explanations account for this failure of attitude to change in line with value modification. First, the validity of the attitude scale must be questioned in light of students' written comments. Many respondents resented the obtrusive measurement of their attitude toward a minority group as a whole. Written comments of this type included the following: "I can't categorize people. I wouldn't want it done for my race and I don't think they appreciate it either." "Blacks are like anyone else. They have a propensity to be all of the above the same as anyone else." Other respondents indicated a lack of familiarity with the black race, a unique characteristic of this population. Comments of this type included the following: "In my life I've only known one black when I was six years old. I like them but do not feel qualified to answer." "I know very few blacks and have not looked upon them in these terms." Such resentments and ignorance possibly interfered with the scales' ability to tap attitudes toward this minority group. Second, this scale may be invalid because it assumes stability of attitude toward an object across all situations. If Rokeach's explanation[10] of a two attitude system is correct, individuals possess different attitudes toward objects and the situations in which they are found. The failure of the Remmers scale to register attitudinal changes may be a consequence not only of the failure of this particular scale, but of the measurement procedure.

Three problems with the behavioral criterion may account for the lack of behavioral change. First, the letters were mailed to participants the weekend before finals week, spring quarter. The participants may have been so involved in test-taking and preparations for leaving that their commitment to a new organization to be formed fall quarter may have been low, or they may have forgotten to mail back their answers because of other obligations. Second, conceptual problems may account for the

[9] Gollob and Dittes.

[10] Milton Rokeach, Beliefs, Attitudes, and Values (San Francisco: Jossey-Bass, 1968).

failure. Fishbein[11] argued that behavioral criteria like any attitude scales should be tested for validity and reliability. Reliability and validity for this criterion were based on its presumed similarity to Rokeach's measuring instrument. This presumption may not have been justified. Third, Doob[12] and Thurstone[13] argued that an attitude may mediate several different behavioral responses. Rokeach[14] also argued that behavior was dependent both upon an individual's attitude towards an object and his attitude toward the situation in which he/she confronts the object. Fishbein[15] proposed that several behaviors across different situations need to be observed to determine the overall relationship between attitude and behavior. Since values can be classified as highly general behavioral orientations, the same conclusions should apply to determining the relationship between value modification and behavioral change.

The most striking finding with regard to hypothesis one is that a persuasive appeal based on threatened self-esteem which made no direct reference to value inconsistency provided stronger motivation for individuals to modify their values towards consistency than did a message directly making individuals aware of the value inconsistency. A persuasive method which concentrates its appeal upon values without directly relating them to the audience's self-esteem, then, has less chance of producing desired cognitive and behavioral changes than a message which directly involves these self-oriented factors.

Hypothesis Two

Contrary to predictions from dissonance theory, overall levels of self-esteem did not influence the magnitude of value modification. These findings suggest that an immediate threat to self-esteem is a compelling reason for change no matter what an individual's overall self-evaluation. This conclusion was supported by Gollob and Dittes[16] who also found that manipulated and not manifest self-esteem determined the magnitude of attitude change. This finding, although surprising, adds to the effectiveness of the persuasive approach being discussed. The personality characteristics, since they are not influential, need not be measured or taken into account when preparing a persuasive message intended for a large group of people.

Hypothesis Three

Rokeach's assumption that value modification, once obtained, will persist over time is supported by this research. Even after hearing the two less successful persuasive messages, individuals maintained their initial value modification six weeks after the persuasive messages were heard. Individuals in the more successful condition, where the persuasive message emphasized a strong and unambiguous threat to self-esteem, changed their values. These value changes not only persisted but increased during the six weeks between posttests. These findings lead to the conclusion that a self-persuasion process operates after the initial persuasive attempt.

Implications

Although procedural and measure-

11 Martin Fishbein, "Introduction: The Prediction of Behaviors from Attitudinal Variables," in C. C. Mortensen and K. Sereno (Eds.), *Advances in Communication Research,* (New York: Harper and Row, 1973).

12 Leonard W. Doob, "The Behavior of Attitudes, *"Psychological Review,* 54 (1947), p. 135-156.

13 L. L. Thurstone, "The Measurement of Social Attitudes," *Journal of Abnormal and Social Psychology,* 26 (1931), p. 249-269.

14 Rokeach, *Beliefs, Attitudes, and Values.*

15 Fishbein.

16 Gollob and Dittes.

ment problems make it impossible to confirm or disconfirm Rokeach's assumption that value change will lead to corresponding changes in relevant attitudes and behaviors, implications from the differential value changes across message conditions and over time add new insight into a discussion of persuasive strategies and motivation for cognitive changes. The fact that a challenge to self-esteem produced the greatest value reorganization toward consistency suggests strongly that Pilisuk's rather than Rokeach's explanation for persuasion is the most cogent. Individuals who feel they are being evaluated negatively on moral or intellectual grounds seek to reinstate a more positive image to the persuader and/or to themselves by realigning their value priorities in a more positive ordering.

The strength of this finding also leads to a possible reinterpretation of Rokeach's original persuasive method. An alternative explanation for the effectiveness and stability of the inconsistent value and the inconsistent value and negative self-esteem messages is that respondents perceived an implicit negative evaluation within the two messages and changed their value rankings accordingly. A second competing explanation is that demand characteristics of the experiment caused respondents to demonstrate more consistent rankings in accord with perceived experimenter expectations. The latter explanation appears less likely, however, since all value changes either persisted or increased over time. If pleasing the experimenter was the main motivator, respondents would be expected to regress toward their original value rankings over time.

The fact that individuals' levels of overall self-esteem made no difference in the amounts of change justifies a strong reinterpretation of the dissonance assumption that people with high self-esteem will be more motivated to protect their image than those with low self-esteem. Apparently Rokeach's assumption that the self-concept is part of an individual's personality core is strongly supported for all people, not just those with high self-esteem.

While the small number of participants in each cell warrants a tentative conclusion, a persuader would be well advised to consider the following three suggestions when planning a persuasive message: (1) include a challenge or threat to the self-esteem of audience members within the overall persuasive appeal, (2) include the audience's value inconsistencies within the appeals to self-esteem, and (3) connect all persuasive appeals to highly specific and relevant attitudes and behaviors which follow directly from the self-esteem and value appeals contained within the persuasive message. Researchers would be well advised to follow several suggestions in designing and measuring relevant cognitive and behavioral changes within their persuasive experiments. Although not new, this study reemphasizes the need to (1) determine the amount of error and change that result from demand characteristics of the experiment rather than experimental controls and manipulations, (2) use a more specific rather than a global measure for attitude and make sure that the measure has been adequately validated for the specific population in question, and (3) include a behavioral measure which has been adequately validated and which does not simply incorporate a single measure of one behavior at one point in time. Both Fishbein's and Rokeach's suggestions for the measurement and validation of attitudinal and behavioral measures should be taken seriously in future persuasion experiments.

THE EFFECTS OF SUBSTANTIVE AND AFFECTIVE VERBAL CONFLICT ON THE QUALITY OF DECISIONS OF SMALL PROBLEM SOLVING GROUPS

Mae Arnold Bell

THE traditional theoretical perspective provided by textbooks of group discussion seems to share some of the philosophical underpinnings of confrontation theory of nineteenth century England, represented by spokesmen such as Walter Bagehot and John Stuart Mill. For example, in his essay, "The Age of Discussion," Bagehot explains:

> It is easy to see why the common discussion of common actions or common interests should become the root of change. The mere putting up of a subject to discussion, with the object of being guided by that discussion, is a clear admission that that subject is in no degree settled by the established rule.[1]

Or as Mill expressed a similar sentiment: ". . . the only way in which a human being can make some approach to knowing the whole of a subject is by hearing what can be said about it by persons of every variety of opinion, and studying all modes in which it can be looked at by every character of mind."[2]

Recent theoretical and empirical works focusing on the processes of group interaction reinforce that perspective. Collins and and Guetzkow articulate the theoretical underpinning of the assumed advantage group decision making offers: more communication leads to greater information exchange and more critical response to that information.[3] A number of studies have attempted an empirical test of the assumption that conflict of ideas leads to higher quality decisions. Torrance,[4] Hall and Watson,[5] and Bower[6] provide evidence that the presence of conflict of ideas during the group's deliberations results in higher quality solutions. Hall and Williams conclude, "Kelley and Thibaut (1954) have suggested that individuals are likely to reexamine their opinions only when it becomes obvious that they differ from those of others. . . . It would appear that lack of conflict attentuates group performance . . . with the effect that the group's fullest potential is not realized."[7]

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1 Walter Bagehot, *Prysics and Politics* (New York: Alfred A. Knopf, 1948), p. 166.

2 John Stuart Mill, "Of the Liberty of Thought and Discussion," in *On Liberty*, ed. Currin V. Shields (New York: The Bobbs-Merrill Co., Inc., 1956), p. 25.

3 Barry E. Collins and Harold Guetzkow, *A Social Psychology of Group Processes for Decision-Making* (New York: Wiley & Sons, 1964).

4 Paul E. Torrance, "Function of Expressed Disagreement in Small Group Processes," *Social Forces*, 35 (1957), 314-318.

5 Jay Hall and W. H. Watson, "The Effects of Normative Intervention on Group Decision-Making Performance," *Human Relations*, 23 (1970), 299-317.

6 J. L. Bower, "Group Decision-Making: A Report of an Experimental Study," *Behavioral Science*, 10 (1965), 277-289.

7 Jay Hall and Martha Williams, "A Comparison of Decision-Making Performances in

Most group discussion textbooks share that perspective by including John Dewey's *Five Steps to Reflective Thinking*,[8] which emphasize that a best solution will emerge out of the variety of positions (or solutions) initially proposed through systematic, rational discourse. Thus, one communication variable—communication characterized as reason-giving discourse of divergent ideas—seems theoretically related to making good decisions. Likewise, this emphasis on rational discourse suggests that a contrasting approach, irrational discourse which would not focus on reasons but emotions, is detrimental to effective decision-making.

Another communication variable implicit in such rational decision-making models is flexibility. The Dewey model, for example, stresses that all the plausible solutions, and some not so plausible, should be explicated before the group, as individual members or as a whole, begin movement toward one specific solution that becomes their final choice. A number of experimental and field studies[9] reinforce Dewey's suggestion that diversity of possible solutions early in a group's deliberations, i.e., its remaining open to several possibilities, results in a better final decision.

A study by Marr[10] suggests a third variable, sex, which may confound the relationships between rationality, flexibility, and quality decisions in a small group setting. His results indicate possible sex differences for both rationality and flexibility: "The results of this study indicate that females tend to compromise more than males. Furthermore, males apparently are less logical and more emotional in their conciliatory responses to verbal statements."[11]

Although the quality of groups' decisions has served as a perennial focus for empirical investigation, few studies begin with the assumption that conflict of ideas contributes to the quality of the emerging synthesis of ideas. For example, most investigations do not focus primarily on the communication of groups as a factor influencing the quality of solution. Or, if communication is a factor, attention is devoted to structural variables rather than process variables which focus on the form and function of actual communication of small groups. Such structural variables as individual versus group solutions,[12] type of task,[13] opportunity to communicate as defined in centralized/decentralized networks[14] have served as foci for some extended lines of research.

A few studies do focus on the group's communication defined as a process variable and quality of decision. In his

Established and Ad Hoc Groups," *Journal of Personality and Social Psychology*, 3 (1966), 221.

[8] For example, see Dennis Gouran, *Discussion: The Process of Group Decision-Making* (New York: Harper and Row, 1974) 24.

[9] A representative experimental study would be Milton Shaw and W. Penrod, "Validity of Information, Attempted Influence, and Quality of a Group Decision," *Psychological Reports*, 10 (1962), 19-23; a representative field study is Paul E. Torrance, "Group Decision-Making and Disagreement," *Social Forces*, 35 (1957), 314-318; a representative documentary study would be Irving Janis' *Victims of Groupthink* (Boston: Houghton Mifflin Co., 1972).

[10] Theodore J. Marr, "Conciliation and Verbal Responses as Functions of Orientation and Threat in Group Interaction," *Speech Monographs*, 41 (1974), 6-18.

[11] *Ibid.*, 17.

[12] Shaw provides a comprehensive review of social psychological research in Group Dynamics: *The Psychology of Small Group Behavior* (New York: McGraw-Hill, 1976), 58-68.

[13] R. N. Kent and J. E. McGrath, "Task and Group Characteristics as Factors Influencing Group Performance," *Journal of Experimental Social Psychology*, 5 (1969), 429-440, or Arthur P. Bochner, "Special Reports: Task and Instrumentation Variables as Factors Jeopardizing the Validity of Published Group Communication Research, 1970-1971," *Speech Monographs*, 41 (1974), 169-178.

[14] H. J. Leavitt, "Some Effects of Certain Communication Patterns on Group Performance," *Journal of Abnormal and Social Psychology*, 46 (1951), 38-50, or M. E. Shaw, "Some Effects of Prolonged Experience in Communication Nets," *Journal of Applied Psychology*, 40 (1956), 38-50.

review of group effectiveness, Bass concludes the group productivity is dependent on the effectiveness of group interaction.[15] That relationship between the effectiveness of the communication or interaction of the group and quality decisions is not clearcut, however. Several studies provide support for the link between effective communication and quality decisions,[16] and others do not provide such evidence.[17]

Most of the experimental studies cited earlier had the groups solve problems for which the best or correct solution was determined by a panel of "experts" in the problem's content area.[18] The qualifications entitling the experts to their status are rarely provided. When they are included, some question could be raised regarding the possibility of establishment and other biases being introduced. To avoid justifying the choice of experts, the problem chosen for this study had only one correct solution, verifiable mathematically. Admittedly many of the problems which small groups attempt to solve do not have one clearly best solution, but the assumption is that the processes which would lead a group to one of the judgmentally best solutions should be equally operative in a situation in which the correct solution could be ascertained.

The preceding review of investigations of the quality of groups' final decisions suggested a rationale for a study of group problem solving to determine whether the manner in which members of a decision making group communicate with one another is associated with the quality of the decisions they reach. Specifically, the following study was designed to measure the impact of conflicting substantive (rational) and affective (irrational) verbal communication for both sexes on both the quality of the final decision and the flexibility of the group members during their deliberations.

Procedures

The experiment consisted of a simulated discussion of the "horsetrading problem" designed by N. R. F. Maier and Allen R. Solem.

> A man bought a horse for $60 and sold it for $70. Then he bought it back for $80 and sold it again for $90. How much did he make or lose in the horsetrading business?[19]

The procedures employed replicated three earlier investigations[20] which indicated that 33 percent of the college freshmen participating would not choose the correct answer, "made $20," but one of the following three incorrect answers: "broke even," "made $10," and "made $30."

One hundred fifty-four university undergraduate students enrolled in two

15 Bernard M. Bass, "The Definition of a 'Group,'" in *Small Group Communication: a Reader*, ed. Robert S. Cathcart and Larry A. Samovar (Dubuque, Iowa: William C. Brown Co., 1975), 24-27.

16 John W. Thibaut and L. H. Strickland, "Communication, Task Demands, and Group Effectiveness," *Journal of Personality*, 28 (1960), 156-166; Dale G. Leathers, "Quality of Group Communication as a Determinant of Group Product," *Speech Monographs*, 39 (1972), 166-173.

17 Judee K. Burgoon, "Unwillingness to Communicate as a Predictor of Small Group Discussion Behaviors and Evaluations," *Central States Speech Journal*, 28 (1977), 122-133.

18 For example, see J. A. Cammalleri, *et al.*, "Effects of Different Leadership Styles on Group Accuracy," *Journal of Applied Psychology*, 57 (1973), 32-37.

19 Norman R. F. Maier and Allen R. Solem, "The Contribution of a Discussion Leader to the Quality of Group Thinking: The Effective Use of Minority Opinions," in *Group Dynamics: Research and Theory*, ed. Dorwin Cartwright and Alvin Zander, 1st ed. (White Plains, New York: Row, Peterson, 1953), 561-572.

20 *Ibid.*; Mae Arnold Bell, "The Effects of Substantive and Affective Conflict in Problem-solving Groups," *Speech Monographs*, 41 (1974), 19-23; L. R. Hoffman and M. R. F. Maier, "Sex Differences, Sex Compositions, and Group Problemsolving," *Journal of Abnormal Social Psychology*, 63 (1961), 453-456.

basic communication courses volunteered to participate. Two groups of four persons each were scheduled for each session from two different classes to assure that no participant would be acquainted with all seven other experimental volunteers: the possibility existed that the other three "persons" in his/her group were strangers. Although each participant was led to believe he or she was communicating with three other people in the room, actually each person received one of the following four sets of messages prepared in advance by the investigator: high-substantive/high affective; high-substantive/low-affective; low-substantive/high affective; low-substantive/low-affective. All four sets of programmed messages were modeled from actual responses of pilot participants, but with the investigator controlling for level of substantive and affective conflict. The stimulus messages of the high substantive condition were operationally defined as "highly substantive if it offers a mathematical procedure indicating how the source of the response arrived at the answer." The low substantive condition did not offer any mathematical procedure or explanation. The high affective stimulus messages contained antagonistic words toward either the group's other participants or toward their ideas; the low affective condition contained no antagonism. An example of one such combination is the first message in the high-substantive condition:

> Your logic is wrong. Stop moving the numbers around and listen. Look, he sold the horse for a $10 profit, then he bought it back and lost that $10 because it cost him $10 more than he sold it for. Now, that's obvious he made $10 profit. If you can't see that, something is wrong with you.[21]

[21] For examples of one set of messages fitting each combination, see above article by Bell.

The same procedure was followed for each hour's group of participants. After the investigator had acquainted them with the use of the cathode ray tube computer terminals, she explained that the purpose of the experiment was to study groups' verbal problem-solving. The confounding influence of visual and related nonverbal cues was offered as one reason for the use of the computer terminals. Next was the statement of the horsetrading problem, the rules for communicating with each other, and the following reward schedule for final choices:

> If everyone in your four-person group agrees on the correct answer, each will receive $1.50 for participating. If you are the only person in your group to get the correct answer, you will receive $1.00 for participating and the other three will receive nothing. If all persons agree on the same incorrect answer, each participant will receive $.50. Otherwise, none of you will receive any money.

The participants were given two minutes to decide on one of the four options provided for their initial individually determined answers. Having typed in their answers, they were told that the computer had tallied their choices and the results were displayed for all four "players." For every participant, all four possible answers were displayed, the three fictitious players choosing the three not chosen by the naive participant. The naive participant and the three "players" then began exchanging messages for a total of twenty responses, each being told by the computer when to respond so that, according to the instructions, each one was assured a "turn" before the others continued to send messages. The naive participant was free to type in whatever he or she wished to communicate. Participants were randomly assigned to one of the four message conditions. After they had communicated, participants made their

second and final choices. The dependent variables to be analyzed were the change to final correct solution and the flexibility, i.e., the number of different solutions a person entertained during the simulated discussion. Regarding the correctness of the final solution, the focal question of this study is whether the kind of communication individuals receive during decision-making contributes to their choice of the correct solution by the time the final choice is made. The assumption is that in a highly substantive discussion in which negative affect is not present, group members, upon hearing the correct solution (and in all conditions either the naive participant or a fictional participant supported the correct solution) will be more likely to change to that choice than group members in other conditions. Related to that hypothesis is one which implies that in a highly substantive discussion with no negative affect, group members who initially choose the correct solution will be more likely to continue to choose it throughout.

Similarly, the hypotheses concerning the flexibility of participants assume that individuals are more likely to switch to another person's answer—even as a gesture of conciliation, not necessarily because they believe the other's answer to be correct—if the deliberations are characterized as highly substantive and not overtly antagonistic.

Two hypotheses were generated for each dependent variable. Hypotheses concerning final choice:

1. Verbal behavior high in substantive content evokes more changes to the correct solution than verbal behavior low substantive content.
2. Verbal behavior low in affective content evokes more changes to the correct solution thar verbal behavior high in affective content.

Hypotheses concerning flexibility:

3. Verbal behavior high in substantive content evokes a greater degree of flexibility than verbal behavior low in substantive content.
4. Verbal behavior low in affective content evokes a greater degree of flexibility than verbal behavior high in affective content.

Although the Marr study was suggestive of the possible impact of a sex difference on the above relationships, the lack of corroborative evidence led to no specific hypotheses regarding sex on change to correct choice or flexibility.

Results

Neither hypothesis concerning correctness of final choice was confirmed. Table 1 presents the results of the two-way chi square analysis for Hypothesis 1. The Pearson χ^2 statistic was .31.

TABLE 1

Chi Square Analysis of Final Correct Choice by Substantive Verbal Conflict Conditions

	Initial Correct to Final Wrong	Initial Wrong to Final Wrong Choice	Initial Wrong to Final Correct Choice	Initial Correct to Final Correct Choice	Row Total
Low Sub.	8	34	12	21	75
Hi Sub.	4	45	14	16	79
Col. Total	12	79	26	37	154

$\chi^2 = .31$, df = 3, n.s.

Table 2 presents the results of the two-way chi square analysis for Hypothesis 2.

The analysis of the flexibility of the participants' choices during the interaction confirmed one of the hypotheses. The χ^2 analysis in Tables 3 through 5 indicated that participants receiving high substantive communication changed choices significantly more than participants receiving low substantive communication. The Pearson χ^2 statistic is 8.06, $p < .05$. Further analysis according to sex indicated that the male participants account for the significant difference. Women in the low-substantive conditions were equally likely to change choices during the interaction as women in the high-substantive conditions. Men, on the other hand, are more likely to change in the high than in the low-substantive conditions. The χ^2 statistic for Table 5 is 11.15, $p < .01$.

The other hypothesis concerning flexibility—that low affective verbal behavior evokes greater flexibility than high-affective verbal behavior—was not confirmed. Table 6 contains the χ^2 analysis; the Pearson χ^2 was .47. Nor was the difference comparing the sexes in the affective conditions significant.

Discussion

The results concerning the movement toward correct solution and flexibility of

TABLE 2

Chi Square Analysis of Final Correct Choice by Affective Verbal Conflict Conditions

	Initial Wrong to Final Right	Initial Wrong to Final Wrong	Initial Right to Final Right	Initial Right to Final Wrong	Row Total
Low Aff.	43	16	21	6	86
Hi Aff.	36	10	16	6	68
Col. Total	79	26	37	12	154

$\chi^2 = .90$, df = 3, n.s.

TABLE 3

Chi Square Analysis of Substantive Communication Conditions by Flexibility

Sub. Condition	No Change	One Change	Two or More Changes	Row Total
Low Substantive	43	14	18	75
High Substantive	32	31	16	79
Column Total	75	45	34	154

$\chi^2 = .12$, df = 2, $p < .05$.

TABLE 4

Chi Square Analysis of Female Participants' Flexibility by Substantive Conflict Conditions

Sub. Condition	No Change	One Change	Two or More Changes	Row Total
Low Substantive	16	10	9	35
High Substantive	15	13	8	36
Column Total	31	23	17	71

$\chi^2 = .79$, df = 2, n.s.

TABLE 5

CHI SQUARE ANALYSIS OF MALE PARTICIPANTS' FLEXIBILITY BY SUBSTANTIVE CONFLICT CONDITIONS

Sub. Condition	No Change	One Change	Two or More Changes	Row Total
Low Substantive	27	4	9	40
High Substantive	17	18	8	43
Column Total	44	22	17	83

$\chi^2 = .004$, df = 2, $p < .01$.

TABLE 6

CHI SQUARE ANALYSIS OF FLEXIBILITY BY AFFECTIVE CONFLICT CONDITIONS

Sub. Condition	No Change	One Change	Two or More Changes	Row Total
Low Affective	41	27	18	86
High Affective	34	18	16	68
Column Total	75	45	34	154

$\chi^2 = .79$, df = 2, n.s.

choices during the interaction, taken together, raise some conceptually interesting issues. The significant finding that individuals are more open to changing their original choices when the communication is highly substantive than when it is not substantive are encouraging and consistent with several studies which indicate that consensus (for which some group members must be willing to move toward others' choices) is related to orientation behavior.[22] The fact that this flexibility did not also result in significantly more correct solutions in the high-substantive conditions is somewhat disconcerting. Clearly, the emphasis of the field of speech communication on groups' reaching consensus assumes that those consensual agreements are also "good" solutions. That assumption is not supported by the results of this study. While the experimental limitations may account for the lack of significant movement toward the correct answer—such obvious limitations as no face-to-face interaction and no possibility for persuasion adapted specifically for the persons involved—the results of this study do suggest that the mere hearing of a substantive explanation supporting the correct solution is not sufficient.

The significant results between men and women's responses to the experimental communication conditions are neither encouraging nor entirely consistent with earlier related research. The results—that women participants are as likely to select more than one solution in the low-substantive communication conditions as in the high-substantive conditions—appears to be yet another study supporting the view that women are generally more persuasible than men. In this instance, the women were as likely to change their minds when no substantive reasons were offered to do so as when good reasons were offered.

[22] Studies include Dennis S. Gouran, "Variables Related to Consensus in Group Discussions on Questions of Policy," *Speech Monographs*, 36 (1969), 387-391; Thomas J. Knutson, "An Experimental Study of the Effects of Orientation Behavior on Small Group Consensus," *Speech Monographs*, 39 (1972), 159-165; Theodore Marr, "Concilliation and Verbal Responses as Functions of Orientation and Threat in Group Interaction," *Speech Monographs*, 41 (1974), 6-18.

The male participants were more discriminating. These results are not a contradiction to the Marr study cited earlier. In that study the groups' task contained no "right" answer, other than one the group members could simply compromise on in order for most of them to make some money. Therefore, flexibility in choice of solution was rational; consequently, Marr's interpretation of his results is that women participants acted more rationally than their male counterparts. In this study, however, flexibility *per se* is not rational, only to the degree that such flexibility leads to selecting the correct solution. On the post-experimental questionnaire, all participants indicated that their strategy during the interaction was that everyone should get the right answer and each make $1.50. Changing from answer to answer, without substantive support for such changes, which is descriptive of the women participants' behavior, does not appear to be rational behavior contributing to their expressed goal. Such conciliatory changes in choice might contribute to everyone's arriving at consensus, but not to each person's earning the maximum financial reimbursement. The men's increase in change of choice only in the high-substantive conditions implies that they were responding to the reasons offered for other answers, not to be conciliatory.

The nonsignificant results concerning flexibility between the affective communication conditions is consistent with Marr's research—neither men nor women participants' flexibility changes significantly as a function of differences in affective communication. These results are somewhat encouraging in that at least the sexual stereotype of men in our society as being more aggressive than women, particularly in highly affective situations, is not supported. For both men and women, the affective nature of the communication situation did not significantly affect the flexibility of choice. Of course, the creation of affective conflict in both Marr's study and the present investigation is open to the challenge that mere words cannot reasonably approximate this type of conflict, which appears to be associated with complex rational and nonverbal variables, at least more so than substantive conflict.

The findings of the study are tentative, at least partially because of the strictly controlled environment in which the communication occurred. Nevertheless, the results are suggestive of further research, using perhaps the same basic variables, but observed in not only laboratory groups (collections of strangers) communicating in face-to-face interaction, but also established groups in which at least some strategies of persuasion already exist for probably both kinds of communicative situations—those characterized by substantive exchanges and by affective exchanges. Identification of such differences in conversation initiated to solve groups' problems would contribute to testing that generalization of Bagehot and Mill: that a major determinant of group performance is the degree to which information can be exchanged among the members of the group.

DEVIATION: A COMMUNICATION PERSPECTIVE

Kathleen Reardon Boynton

SOCIOLOGICAL theories of deviance have focused upon the processes involved in defining an individual as "deviant" as well as the processes by which deviant persons are prevented from seriously threatening the social system in which they function. Both the processes of "deviant typing" and those of controlling the deviant are dependent upon the existence of rules which are consensually shared expectations for behavior existing to maintain the status quo. According to some theorists, like Howard Becker, it is these rules that make deviation possible.[1] Obviously, without rules there would be no rule breakers. But without rules there would also be very little predictability and control of individuals' behaviors and thereby very little social certainty.[2]

Rubington and Wienberg define deviance as "an implied breach of social rules".[3] Social certainty is threatened by deviance and so on the societal level highly formalized agencies of social control exists to bring the deviant back into the predictable and orderly routine expected of him or her by other societal members. These procedures constitute the process of systemic "equilibation."[4] That is, the society seeks to preserve itself and therefore establishes what Laszlo refers to as "resources of control"[5] to ensure that all threats to the system are acted upon before irreparable damage occurs. Rules are also systemic resource of control. They exist to prevent having to resort to corrective measures. But when they fail, societies must turn to other means of maintenance or suffer possible irreversible alterations.[6]

Early conformity research such as that of Allport, 1924[7] and Sherif, 1935[8] led to a series of studies conducted primarily in the 1960's which revealed that groups as well as societies have inherent resources of control which insure the orderly and predictable behavior of members.[9] Kelley in 1952[10] and later Deutsch

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1 H. S. Becker "On Labeling Outsiders" in *Deviance/The Interactionist Perspective,* ed. by E. Rubington and M. S. Weinburg (New York: The MacMillan Company, 1968). 14.

2 See A. Birenbaum and E. Sagarin, *Norms and Human Behavior* (New York: Praeger Publishers, 1976).

3 E. Rubington and M. S. Weinberg, 13.

4 T. Parsons, "A Paradigm for the Analysis of Social Systems and Change" in *Theories of Society,* ed. by T. Parsons, K. D. Naegele and T. R. Pitts (New York, 1961).

5 E. Laszlo, *Introduction to Systems Philosophy,* (New York: Harper and Row Publishers, 1972).

6 Laszlo, 1972, 104-105.

7 F. H. Allport, *Social Psychology,* (Boston: Houghton Mifflin, 1924.

8 M. Sherif, "A Study of Some Social Factors in Perception," *Archives of Psychology,* 1935, 27, No. 187.

9 M. Deutsch and H. Gerard "A Study of Normative and Informative Social Influences on Individual Judgment," *Journal of Abnormal and Social Psychology,* 1955, 51, 629-636. H. B. Gerard, "Deviation, Conformity, and Commitment," in *Current Studies in Social Psychology,* ed. by I. D. Steiner and M. Fishbein (New York: Holt, Rinehart, and Winston, 1965), pp. 263-276. C. A. Kiesler and L. H. Corbin, "Commitment, Attraction, and Conformity," *Journal of Personality and Social Psychology,* 1965, 2, 890-895. C. A. Kiesler, M. Zanna and J. DeSalvo, "Deviation and Conformity: Opinion Change as a

and Gerard in 1955[11] proposed that people use groups to gain and maintain acceptance and/or to gain information. Subsequent research sought to uncover those factors influencing normative and informative group functions.

If, as Kelley posited, people need groups for acceptance and information, then groups could exert pressures on individuals to conform to expectations. Social psychologists described two types of conformity-compliance and private acceptance resulting from such pressure.[12] Studies focusing on goal clarity,[13] relevance,[14] and group capacity of attainment,[15] favors,[16] choice conflict,[17] status,[18] attraction,[19] future commitment,[20] and deindividuation,[21] emerged providing a repertoire of variables which affect individual member decisions to conform to or deviate from group expectations.

Deviation research on the societal level and conformity research on the group level have paved the way for the theoretical model of dyadic interaction to be proposed in this paper. It is likely that dyads also serve normative and informative functions and, furthermore, the variables operating within the dyad influencing an interactant to conform or deviate are not completely separate from those of the society in which it occurs or the reference groups with which its interactants are affiliated. However, there is compelling evidence to suggest that the dyad is a substantially different context from the larger systems which surround it and, therefore, the rules applicable and the resources of control available to its participants differ markedly.[22]

The model to be proposed here has its greatest implications for the study of dyadic conversation. Deviations which occur on this level infrequently have the far-reaching effects of deviations on the group and societal levels. Moreover, the dyad lacks the deindividuation of larger systems and is instead an environment

Function of Commitment, Attraction, and the Presence of a Deviate, *Journal of Personality and Social Psychology*, 1966, 3, 458-467. C. A. Kiesler, S. B. Kiesler, and M. S. Pollak, "The Effect of Commitment to Future Internation on Reactions to Norm Violations," *Journal of Personality*, 1967, 35, 585-599.

10 H. H. Kelley, "Two Functions of Reference Groups" in *Readings in Social Psychology*, ed. by G. E. Swanson, T. M. Newcomb and E. L. Hartley (New York: Holt, Rinehart and Winston, 2nd edition, 1952).

11 Deutsch and Gerard, 1955.

12 See C. A. Kiesler and S. B. Kiesler, *Conformity* (California: Addison-Wesley Publishing Company, 1970).

13 B. H. Raven and J. Rietsema, "The Effects of Varied Clarity of Group Goal and Group Path Upon the Individual and His Relation to the Group," *Human Relations*, 1957, 10, 29-44.

14 S. Schachter, Deviation, Rejection and Communication," *Journal of Abnormal and Social Psychology*, 1951, 46, 190-207.

15 E. J. Thomas, "Effects of Facilitative Role Inter-dependence on Group Functioning," *Human Relations*, 1957, 10, 347-366.

16 E. E. Jones, *Ingratiation* (New York: Appleton-Century-Crofts, 1964). J. W. Brehm and A. Cole, "Effects of a Favor which Reduces Freedom," *Journal of Personality and Social Psychology*, 1966, 3, 420-426.

17 H. B. Gerard, 1965. L. Festinger, *Conflict, Decision, and Dissonance*, (Stanford: Stanford University Press, 1964).

18 E. P. Hollander, "Conformity, Status, and Idiosyncrasy Credit," *Psychological Review*, 1958, 65, 117-127. H. Meadow and A. Zander, "Aspirations for the Group Chosen by Central and Peripheral Members," *Journal of Personality and Social Psychology*, 1965, 1, 224-228.

19 K. W. Bock, "Influence Through Social Communication," *Journal of Abnormal and Social Psychology*, 1951, 46, 9-23. H. C. Kelman and A. H. Eagly, "Attitude Toward the Communicator, Perception of Communication Content and Attitude Change," *Journal of Personality and Social Psychology*, 1965, 1, 63-78. Kiesler and Corbin, 1965.

20 Kiesler, Kiesler and Pollak, 1967.

21 L. Festinger, A. Pepitone and T. Newcomb, "Some Consequences of Deindividuation in a Group," *Journal of Abnormal and Social Psychology*, 1952, 47, 382-389. N. Kogan and M. A. Wallach, "Effects of Physical Separation of Group Members Upon Group Risk-Taking," *Human Relations*, 1967, 20, 41-48. J. E. Singer, C. A. Bush, and S. C. Lublin, "Some Aspects of Deindividuation: Identification and Conformity," *Journal of Experimental and Social Psychology*, 1965, 1, 365-378.

22 See D. D. Jackson, "Family Rules: Marital Quid Pro Quo" in *The Interactional View*, ed. by P. Watzlawick and J. H. Weakland (New York: W. W. Norton and Co., Inc., 1977). R. K. Leik, "Instrumentality and Emotionality in Family Interaction," *Sociometry*, 26, 1963, 131-145.

in which the individual is highly visible and consistently active.

The goal of this paper, then, is to propose a theoretical model from which studies of conversational deviance at the dyadic level may be generated.

What Is Deviance

One of the major premises of the theoretical model to be proposed here is that persons *choose* to behave in certain ways. These choices are infrequently, if ever, made in the absence of constraints and many of these choices become habituated responses or "functionally autonomous"[23] behaviors which appear to be merely passive responses to stimuli. They are, however, choices guided by the rules which have come to be associated with the context in which the person is functioning. Bateson defines context as "a collective term for all those events which tell the organism among what set of alternatives he must make his next choice."[24] These contexts are repeatable and so the person comes to know through his or her successes and failures in these contexts what behaviors are appropriate. S/he develops repertoire of behaviors which "fit" the context and the role s/he fills in that context.

The context itself is also learned and identified in interpersonal relationships by "signal markers"[25] which indicate to the participants that a certain punctuated sequence of events is likely to ensue. Although the interactants' conceptualizations of what the event or episode should be like may differ due to having had different experiences, they will be more or less able to rely on their coordination and management[26] skills to compensate for such differences.

Since interpersonal interactions are at least to some degree stochastic—each behavior is the response to the one preceeding it as well as the precusor of that which follows—expectations formed from having experienced repeated contexts crystallize into rules but must remain tentative serving only to lend some degree of interpersonal certainty similar to the "social certainty" afforded by social rules.[27]

To treat rules (crystallized expectations) as more than guides to action is to suggest that persons are passive recipients rather than monitors of their own actions. That persons can *intend* to bring about a goal suggests that they have at least some control over their behaviors. The extent of this control depends upon the individual's ability to critique his or her behavior.

Cronen and Pearce[28] have proposed a hierarchy of meanings in interpersonal communications which implies that persons operating at lower levels of the hierarchy have less freedom of action than those persons who can construe episodes or life scripts. They explain that "the ability to construe episodically allows the participants to critique . . . rules and negotiate a shared episode rule permitting more control over the sequence of acts."[29] This extension of Kelly's[30] concept of cognitive complexity suggests that while some persons' behaviors are very much constrained by their rule sets, others can critique their own rules removing them from the role of passive recipients in their interactions.

23 V. E. Cronen and W. B. Pearce, "The Logic of the Coordinated Management of Meaning: An Open Systems Model of Interpersonal Communication," unpublished manuscript, 1977, 20-21.

24 G. Bateson, *Steps to an Ecology of Mind*, (New York: Ballantine Books, 1972), 289.

25 Bateson, 178.

26 W. B. Pearce, "The Coordinated Management of Meaning" in *Exporations in Interpersonal Communication*, ed. by G. Miller (Beverley Hills: Sage, 1976).

27 See Birenbaum and Sagarin, 2-3.

28 Cronen and Pearce, 1977.

29 Cronen and Pearce, 1977, 18.

30 G. A. Kelley, *The Psychology of Personal Constructs* (New York: Norton, 1955).

So, it is by experiencing repeatable contexts that we come to punctuate events as episodes and it is through our successes and failures as participants in these events (episodes) that we learn the rules which guide our behavior and inform us of what may be expected of others. By checking our actions against those of other interactants we can critique our rules and negotiate a more or less tentative contract by which a mutual understanding of what is to occur is derived. What remains after one *negotiates* (implicitly or explicitly) to engage in a particular type of episode (punctuated sequence of events) is the *management* of that episode. Herein lies the potentiality for deviation. Borrowing from Howard Becker the idea that deviance is not a quality inherent on the behavior itself but rather one created by the person who commits an act and those who respond to it,[31] the occurrence of deviation is perhaps best viewed as a form of mismanagement by one or more interactive participants. There is a breakdown of coordination which violates the expectations of at least one of the participants. In the following section the conjoining of systems and rules is discussed. From this metatheoretical perspective a model of episode deviation is derived.

Conjoining Systems and Rules

Any interaction between individuals can be considered a social system. The system is managed by participants who negotiate their intentions to engage in a particular episode or sequence of episodes. Social systems can be analyzed from two different points of view. The first is a morphological one focusing on the structure of the system. Laszlo defines this procedure as one of giving an internal state description of the system. The other approach focuses on dynamic functions of the system. Here internal constraints which give the system its structure are related to external factors.[32]

The approach taken in this paper is the latter—dynamic functions. This perspective implies that the system to be analyzed is open, thus environmental factors are assumed to interact with the internal elements of the system. The environment is not the physical surroundings per se but rather the cultures, personalities and organisms which are themselves in a constant state of flux. Laszlo explains:

> "Thus defined, the social environment is never constant: as cultural norms evolve, personalities and organisms replace one another in a continuous succession in which traits are imperfectly replicated. Hence, the social system finds itself in a sea of change, and is faced with the alternatives of either maintaining itself, or suffering possibly irreversible alterations."[33]

In accordance with Bertalanffy,[34] rules will be defined in this theoretical model as "constraints imposed upon the dynamic interaction of system components" operating as resources of control. It is the rules that are violated when deviation occurs and thus the system is threatened by a lapse in one of its control resources. The model depicts the corrective actions that are taken by the non-deviant interactant to restore the dyadic system's "steady state."

Since modeling a dynamic system is difficult, at best, the image that may emerge from the model alone is static. It is therefore important that the open systems nature of the dyad be remembered throughout this model presentation to avoid misconceptions.

31 Becker, 14.

32 Laszlo, 101.
33 Laszlo, 103.
34 L. von Bertalanffy, *General Systems Theory* (New York: George Brazitler, 1968), 44.

A Model of Episodic Deviation

The theoretical model proposed here focuses on what Lofland refers to as the "situation of deviance among normals" characterized by "isolated or episodic deviant acts" rather than on persons who continuously engage in deviant acts.[35]

The model proposes a series of corrective phases which are enacted by the dyad member after s/he perceived what s/he considers to be deviant behavior on the part of his or her co-interactant. During these phases s/he seeks "sufficient" reason for the other's actions to "account"[36] for what has been perceived as deviation.

Negotiation Phase

The first phase of the Episodic Deviation Model is the episode negotiation phase during which the participants in the episode seek to agree upon the act-action structure(s) or rules of the episode. Such negotiations can take the form of requests, commands, threats, discussions, etc. If the participants choose to engage in a ritual or formal episode, then the rules are probably known to the participants already. Less ritualistic act-action structure will tend toward the enactment of informal episodes and require greater management skills. The participants attempt to agree upon the point at which the episode will commence to be enacted. If the purely ritualistic choice is made, Boynton[37] posits that deviations will cause greater disquilibrium and a greater need to account for them since rituals consist of many obligatory and prohibitive rules. Less formal episodes in which obligations and prohibitions are minimal should experience fewer disequilibrium effects of deviance.

Negotiations between interactants do not commit them to the completion of that episode type. Conversational negotiations are more or less tentative depending once again upon the formal or informal nature of the episode. Most episodes allow for renegotiations and so an encounter that begins as a pleasant discussion episode may be renegotiated to an argument episode through a series of hostile explicit or implicit behaviors.

Deviations may also lead to renegotiations. The conversational segment in Figure 2 involves both an attempt at renegotiation and subsequent deviation. Renegotiations by adults are usually less abrupt than those found in child conversations since adults are more likely to have learned rules for conversational reciprocity and mastered techniques for mutual renegotiation.[38]

Some deviations do not lead to renegotiation or episode termination (see Figure 1). If something unusual occurs during the enactment of an episode it may be perceived as a deviation, in which case the entire model applies. If, however, this infraction of the rules is perceived as not sufficient to warrant interruption of the episode, it may merely be dismissed in the test phase as less than "real" deviation. In other words, not all infractions of the rules will require the enactment of the entire post test phases of the model.

Test Phase

Dittman's concept of scarcity provides a perspective from which to view the

[35] J. Lofland, *Deviance and Identity* (New Jersey: Prentice Hall, Inc., 1969), 27.

[36] R. Harre and P. Secord, *The Explanation of Social Behavior* (New Jersey: Littlefield, Adams and Company, 1972), 89.

[37] K. R. Boynton, "Episodic Deviation a Precursor to Dynamic Homeostasis in Interpersonal Relationships," presented at the Speech Communication Association Convention, December, 1977.

[38] K. R. Boynton and L. L. Henke, "Conversational Expansion in Young Children," *Communication Education*, in press. L. L. Henke and K. R. Boynton, unpublished manuscript, 1977.

FIGURE 1

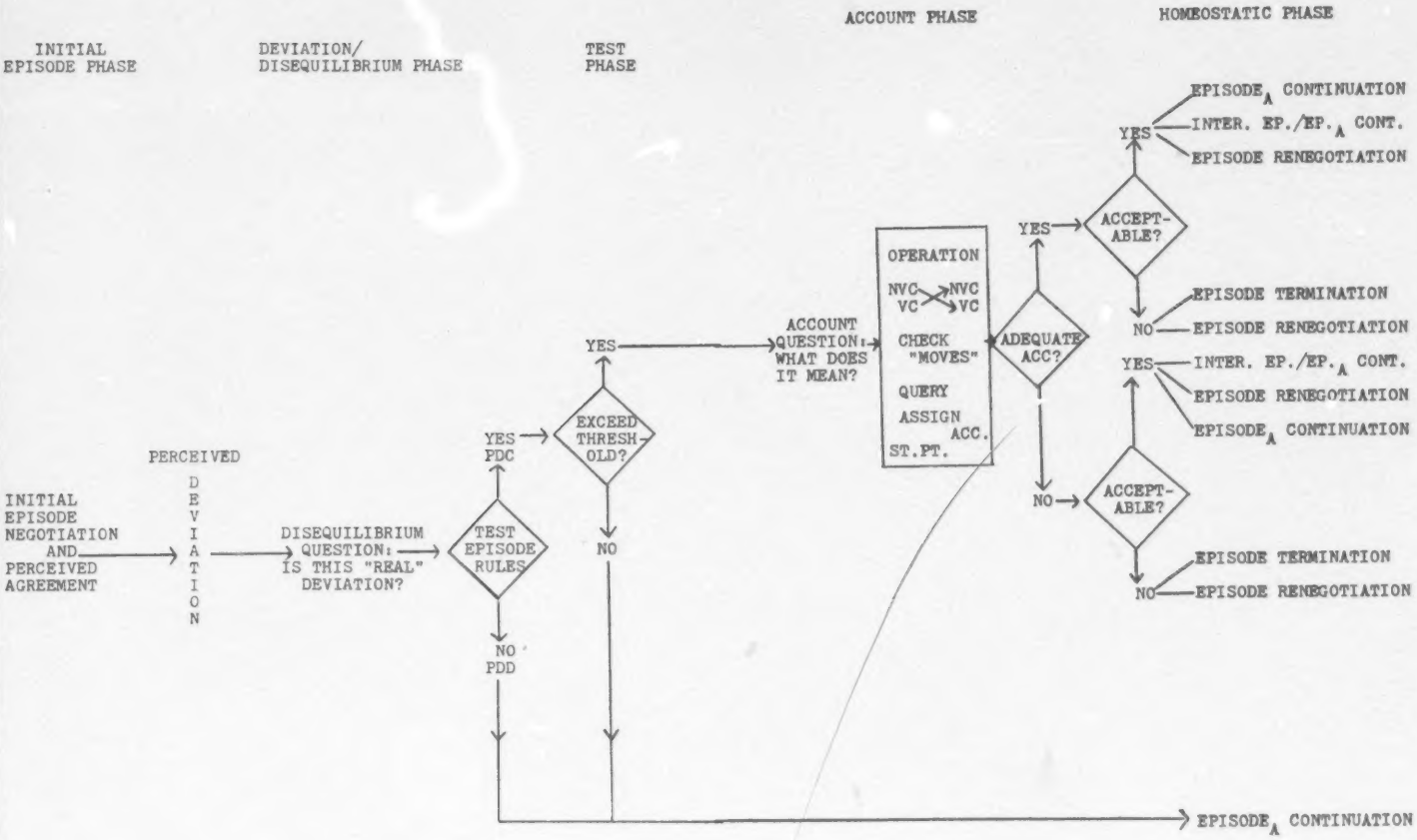
INITIAL EPISODE PHASE
DEVIATION/ DISEQUILIBRIUM PHASE
TEST PHASE
ACCOUNT PHASE
HOMEOSTATIC PHASE
INITIAL EPISODE NEGOTIATION AND PERCEIVED AGREEMENT
PERCEIVED DEVIATION
DISEQUILIBRIUM QUESTION: IS THIS "REAL" DEVIATION?
TEST EPISODE RULES
YES PDC
NO PDD
EXCEED THRESH-OLD?
YES
NO
ACCOUNT QUESTION: WHAT DOES IT MEAN?
OPERATION
NVC → NVC
VC → VC
CHECK "MOVES"
QUERY
ASSIGN ACC. ST.PT.
ADEQUATE ACC?
YES
NO
ACCEPT-ABLE?
YES
NO
EPISODE$_A$ CONTINUATION
INTER. EP./EP.$_A$ CONT.
EPISODE RENEGOTIATION
EPISODE TERMINATION
EPISODE RENEGOTIATION
INTER. EP./EP.$_A$ CONT.
EPISODE RENEGOTIATION
EPISODE$_A$ CONTINUATION
ACCEPT-ABLE?
YES
NO
EPISODE TERMINATION
EPISODE RENEGOTIATION
EPISODE$_A$ CONTINUATION

FIGURE 2

Robin (age 3) and Lisa (age 5) at play

R Honey, come here. Honey come here.
L Ah, Mom. I have to go mommy.
R Honey. Stop, Stop, Stop.
L I have to get to the house I can't walk. I have to get to the house. I can't walk.
R Honey, stop. I'm
L I have to get to the house
R oow

Background—Daddy says "You'll get your toes run over."

L Yes, that's why.
R Honey. Now honey let me change that. Now it's much better
L Be quiet. I'm in a concert. You're watching me. A concert (attempted switch of play focus—abrupt renegotiation)
R Lisa, come on honey. Da ba (denial—deviation from L's expectations)
L Be quiet you're at a concert. (repeat and attempt to coordinate)
(singing) Hey you guys. Robin, we better let your mom um um ta um his let us listen to this cause cause I have to go to the potty and I don't think I'll be able to listen. (attempted switch from play episode to serious episode)

R Are you going to play first?
L Yeh, but when I go to the potty } Renegotiation
R Okay.

test of "real" deviation. He suggests that the information value of emotional messages is measured essentially by its unusualness or scarcity.[39] Unlike situations where there is a finite number of possible "next elements," social situations do not have such neatly limited possibilities. However, there are rather well known unwritten rules which govern "next elements" determined by cultural, regional, familial, and ideosyncratic patterns.

It follows from the preceding that the longer we have lived within a culture, region or family and the longer we have known the person(s) with whom we are interacting, the more capable we should be of recognizing deviations in behavior since we will have developed patterns of rule set interpenetration which establish parameters of acceptable behavior.

Although Dittman discusses scarcity in terms of emotion, the concept is not without relevance for discussions of all behavior deviations. It is presumed that there exists a baseline set of behaviors against which deviations may be measured for their unusualness. Corroborating this conceptualization with that of Cronen and Pearce,[40] the baseline can be regarded as the expectancies resulting from prior repeatedly successful interpenetrations of rule sets for a particular type of episode.

Once the observer has determined to his or her satisfaction that the deviation was indeed "real", then s/he is faced with a decision—whether to excuse it without an adequate account or whether to pursue a justification for such unusual behavior. The basis for this decision is the observer's deviation threshold for this particular episode, this particular deviation, and this particular deviator. In this phase, the observer's past experiences must be called to provide a baseline against which s/he may measure and determine the degree of tolerability for this particular deviation situation. Whether this is the first deviation or one of several is an important consideration since it is likely that the threshold for successive deviations will have been lowered by the discomfort caused by previous deviations. This is consistent with Zillmann's Excitation-transfer theory which suggests that excitation

39 A. Dittman, *Interpersonal Messages of Emotion* (New York: Springer Publishing Company, 1972), 43.

40 Cronen and Pearce, 1977.

decays slowly and so residues may enter into subsequent experiential states.[41]

Although the model does not deal with successive deviation and its effect on deviation tolerance, this is certainly an intriguing area open to investigation. Emotional state, fatigue level, affiliation needs, equity and a number of other variables most likely contribute to the threshold designation.

Account Phase-Operations

Once the observer has determined that the perceived deviation is indeed "real" in terms of episode rules and that it sufficiently exceeds his or her deviation tolerance threshold, then s/he will seek to account for the deviation. An account is a justification of actions achieved by attributing reasons to the action in question.[42] If it is determined that the deviation is merely a matter of habit, a "zero-account"[43] may be attributed. However, if the deviation cannot be accounted for by events outside the control of the deviator, then another account must be located by performing a series of operations. This does not mean that involuntary deviant behavior will always be detected before other operations, although that may be the case. While the order of operations may be a matter of speculation at this point, that such operations exist is well supported.

Attribution Theory and Accounts

Attribution theories suggest that persons seek reasons for the behaviors of others.[44] Schacter[45] contends that the individual who experiences nonspecific physiological arousal initiates a quest for "causal accounting" that will explain his/her emotional reaction and guide his/her response selection.

According to Leventhal,[46] emotionality occurs when an individual is unprepared for an event. If no uncertainty is created, the individual remains calm. Deviation, defined as an abrupt violation of expectations, does create uncertainty and leads, as Leventhal and other attribution theoriests suggest, to an account that restores equilibrium.

Scott and Lyman[47] describe two types of accounts: excuses and justifications. These accounts are derived from the quest for "causal accounting" initiated by the interactant who perceives deviation on the part of his/her co-interactant. The accounts are either provided by the deviator or created by the deviation observer.

As the model suggests, if the perceived deviation was verbal, the observer will seek to account for it by checking the opposite channel—nonverbal and vice versa. Assuming the cross channel check does not provide an account, then other operations will be performed either separately or collectively. In the case where both channels were involved in the deviation these other operations will be called upon to aid in the assignment of an adequate account. If an adequate account is not readily forthcoming, the

[41] D. Zillman, "Attribution and Misattribution of Excitatory Reactions," in *New Directions in Attribution Research* (Vol. 2), ed. by J. H. Harvey, W. J. Ickes, and R. F. Kidd (New Jersey: Erlbaum), in press.

[42] Harre and Secord, 160.

[43] Harre and Secord, 162.

[44] E. E. Jones and K. Davis, "From Acts to Disposition. The Attribution Process in Person Perception" in *Advances in Experimental and Social Psychology*, ed. by L. Berkowitz, Vol. 2 (New York: Academic Press, 1965), p. 220.

[45] S. Schachter, "The Interaction of Cognitive and Physiological Determinants of Emotional State" in *Advances in Experimental and Social Psychology* (Vol 1), ed. by L. Berkowitz (New York: Academic Press, 1964).

[46] H. Leventhal, "Emotions: A Basic Problem for Social Psychology," in *Social Psychology: Classic and Contemporary Integrations*, ed. by C. Nemeth (Chicago: Rand McNally, 1974).

[47] M. B. Scott and S. M. Lyman, "Accounts" in *Symbolic Interaction: A Reader for Social Psychology*, 2nd edition, ed. by J. G. Manis and B. N. Meltzer (Boston: Allyn and Bacon, Inc., 1974).

observer may continue performing separate or combined operations until his or her tolerance level is reached (stop point). This operation tolerance level is a function of many factors. Borrowing from the conformity studies described earlier in this paper, it is likely that attraction to the deviator, status of observer and deviator, commitment to future interaction, number of alternative behaviors, severity of the deviation, etc., are few of the factors that may influence the operation tolerance level. The operation phase is presently in an embryonic state. However, studies on the dyadic level manipulating those variables that have proven influential to conformity within groups is a promising direction for future research.

As the model suggests, the particular operations may include strategic "moves" such as those proposed by Goffman[48] (control, uncovering and counter uncovering) when accounts are not readily accessible.

Account Phase-Acceptability Test

Once the observer has performed operations sufficient to provide an adequate account of the deviation or reached his or her operations tolerance level (stop point), s/he must then decide whether or not s/he accepts his/her findings. An unacceptable account is one that is either illegitimate or unreasonable.[49] For both the adequate account condition and the no account condition the question of acceptability must be answered. Legitimacy and reasonableness are the measure of acceptability in the adequate account condition only. In the no account condition, the absence of an account is considered in light of the relationship and the episode itself and then deemed acceptable or nonacceptable. The decisions to accept or reject an account lead to different outcomes, both are, however, directed at restoring homeostasis.

Dynamic Homeostasis

If Laszlo's[50] description of a system as surrounded and affected by a constantly changing environment is accurate, homeostasis cannot imply a stability like that of a rock but rather a stability which incorporates change—a dynamic homeostasis. In "Episodic Deviation: A Precursor to Dynamic Homeostasis in Interpersonal Relationships"[51] Boynton explains this phenomena and its relationship to deviation. For now it is sufficient to recognize that the following types of homeostasis suggest a new steady state resulting from deviation. This new state may or may not constitute an improvement in the system. In the latter case deviation has served to facilitate progress despite its unsettling effects.

Homeostatic Phase-Adequate Account/ Acceptability or Nonacceptability

The first possible outcome is episode (A) continuation. In this case the perceiver will assimilate the deviation and attempt to proceed with the initially negotiated rules of interaction. This will occur when the account is both quite adequate and very acceptable. The high degree of adequacy and acceptability may be a function of such considerations as the observer's positive perceptions of the deviator. If for example, a student perceives a professor's deviation as accountable and acceptable because of his or her status, then s/he may be willing to concede by assimilation rather than object.

[48] E. Goffman, *Strategic Interaction* (Philadelphia: University of Pennsylvania Press, 1969), 11.
[49] Scott and Lyman, 1974.
[50] Laszlo, 103.
[51] Boynton, 1977.

An alternative outcome for adequate account-acceptability is an intermittent episode followed by Episode A continuation. Such outcomes are especially common when someone inserts a joke into an otherwise serious conversation. In this case the observer interprets the deviation as a cue to play and does so for a time and then returns to the more serious episode.

The third possible outcome of adequate account-acceptability is episode renegotiation. This occurs when the observer decides to interpret the adequate-acceptable account as a cue to change the subject. If s/he too is rather bored or uncomfortable with the subject, renegotiation may occur.

Like the adequate account-acceptable condition, the adequate account-non-acceptability condition may result in one or two possible outcomes. Nonacceptability may also be considered, like acceptability was, in relation to the observer's past and/or present perceptions of the deviator—in this case negative.

If the observer has decided not to accept the account despite its adequacy, s/he will either terminate the episode or renegotiate. Unlike the adequate account-acceptability condition, negative deviator characteristics coupled with the already established discomfort of the deviation condition may suggest that there is not reason (perceived reward) for continuing Episode A. Termination will most likely occur if the observer's emotional reaction to the deviator is more powerful at the time than the reasoned decision to view the account as adequate. Also, if this is the second, third or one of many deviations, despite his or her previous decision to consider it below threshold (during test phase), s/he may at this time decide to consider it an unacceptable deviation. S/he has essentially decided not to risk further discomfort.

Recognition will most likely occur under adequate account-nonacceptability if the observer perceives the continuation of the conversation rewarding (perhaps merely to be sociable) but finds the subject or approach uncomfortable.

Homeostatic Phase-Inadequate Account/ Acceptability or Nonacceptability

Turning now to the inadequate account condition, once again the decision to accept or reject this condition must be made. The decision to accept an inadequate or complete lack of account relies heavily on the pereceived rewards of the continued conversation (or larger relationship). If, for example, the observer feels that the status (superior position) of the deviator somehow excuses him or her from an adequate account, the episode (Episode A) may continue and the unaccounted deviation will be assimilated. This will be especially true when the continuation of an episode despite unaccounted deviation is or may later be rewarding by virtue of the deviator's status (e.g., "I'll let it pass because later he'll help me get ahead").

The intermittent-episode continuation outcome is similar to the identically labelled adequate account-acceptability condition. However, it is more likely in this case that sarcasm may replace pure wit because the unaccounted deviation acceptability has placed the observer in an ingratiating position which s/he may compensate for by brief retaliation under the guise of humor.

Episode renegotiation occurs in the inadequate account-acceptability condition if the observer considers the continuance of the conversation rewarding either presently or some time in the future but considers the inadequate account a cue to change the subject or approach.

We turn now to the inadequate account-nonacceptability condition which is the most discomforting of all the pre-homeostatic conditions. Here the desire to terminate is most likely predominant over the desire to renegotiate. If the observer can find some present or future reward in continuing a conversation with a person whose deviation is not only without accountability but also unacceptable, then s/he may renegotiate and try again. If s/he does so, the next deviation will not be taken lightly because s/he will have already ingratiated himself/herself once in a rather unpromising situation. Essentially the observer who decides to renegotiate under such adverse conditions is saying (perhaps naively), "I trust you will not treat me so again."

Homeostatic Phase-Episode Continuation Without Account

The only outcome in the Hemeostatic Phase of the model is Episode (A) continuation as a result of two possible decisions in the test phase. If the perceived deviation was tested against the rules of the episode and determined merely as perceived and not "real", then Episode (A) continuation will occur without account phase or homeostatic phase occurrence.

The "does not exceed threshold" decision will also result in Episode (A) continuation. In both cases, however, an assimilation period, usually of rather short duration, will occur. Should a second deviation occur during this brief but significant assimilation phase, it is more likely that the second perceived deviation will be considered "real" and perhaps exceed threshold more readily due to the summative disequilibrium effect of adjacent deviations.

The following conversation is an example of the account phase. It includes (1) account offered by T; (2) rejection of initial account by S; (3) a second account attempt by T; (4) account acceptance by S; (5) return account justifying the initial account rejection by S; (6) account acceptance by T. This phase leads to a renegotiation and termination in the homeostatic phase.

FIGURE 3

Conversation between a husband (T) and wife (S)

S Let's sit by the fire (affectionately)
T I have exams to correct
S Oh no you don't (poking her finger jokingly into his chest). We're supposed to have tonight to ourselves. Remember?
T Oh, Damn!
S What! (pulls away abruptly)
T I didn't mean 'Oh Damn' about us. I meant it about the exams. (Account 1)
S No you didn't. You said it because I was being affectionate and you were annoyed. (Account rejection)
T That's not true. Don't tell me what I meant.
S Forget it. I don't need your company that badly. I'm going upstairs.
T Listen, honey. I shouldn't have said that (Account 2). I wasn't thinking (Account 3). I didn't mean it like you took it (Account 4), although I can see why you did. I'll correct them tomorrow (pause) are you going to sit there like a rock. I said I'm sorry.
S Alright (Account acceptance). I wasn't expecting it and it hurt. That's all (Account 1). Besides, I'm really tired (Account 2) and it's been a tough week (Account 3). I guess I'm overly sensitive (Account 4).
T Yeh. That's okay (Account acceptance). I shouldn't have said it anyway.
S I am really tired though (Renegotiation) so I'm going upstairs.
T Me too. I'll correct the exams in school tomorrow. How's that? Then we'll have tomorrow night to ourselves (Renegotiation).
S Okay. (Termination)

Discussion

The theory and model of Episodic Deviation presented in this paper has integrated several previously unrelated areas of study in an attempt to generate some new approaches to the study of human communication. Taking as its foundation the view that persons are active agents in their environment rather than passive recipients, this paper then suggests that this active status is governed by rules which are culturally and personally determined. These cultural and personal rules are learned within situational contexts which over time serve to determine some rules as appropriate and expected in a given interaction situation (episodes) or inappropriate and unexpected (deviant). It is on the latter inappropriate/unexpected behavior situations that this paper primarily focuses. The areas of challenge and also of heuristic value suggested by this theoretic model are described below.

(1) The concept of Deviation Threshold suggests that some perceived deviations are not sufficiently deviant to be troublesome while others plunge the observer into an avid search for reasonable justification (accounts). Future research could be oriented toward discovering factors which influence the threshold designation.

(2) That the degree of deviation is a function of the scarcity of behavior observed as unexpected given the circumstance suggests that for each episode there exist baseline behaviors against which next elements can be measured as appropriate or inappropriate. It was suggested here that this baseline is a product of the "interpenetration" of co-interactant rule sets which become more firmly established as these cointeractants become familiar. This area warrants further investigation.

(3) The operation phase of the model suggests that persons call upon certain reasoning processes to account for deviance within episodes. Although several operations have been suggested in this paper, the ordering of such operations and the existence of other plausible operations is an area for future research.

(4) Future research is needed on the effects of such factors as attraction, status, commitment to future interaction, etc., upon the account phase to determine "why" some accounts are judged adequate or inadequate and acceptable or unacceptable.

(5) The decidedly intrapersonal nature of the model is an attempt to understand the effects of deviation upon one member of a dyad and his or her attempts to restore homeostasis to the dyadic system. A complementary approach is one which focuses upon the system as a whole resulting in an interpersonal model of deviation.

(6) The complementary roles of conformity and deviation within the formal organization is an area recently investigated by Boynton and Fairhurst.[52] Research of this nature on the dyadic and group levels leads to a recognition of those conditions which provide an optimal balance of deviation and conformity in the work environment.

[52] K. R. Boynton and G. T. Fairhurst, "An Elaboration of the Concept Rule: A Case Study with the Military," presented at the International Communication Association Convention, 1978.

Conclusion

The theoretical model proposed in this paper describes deviation "among normals" occurring when persons interact on a dyadic level. The theory and model also provide the necessary groundwork for future research in deviation. This constitutes merely a beginning. The research implications are extensive and there is still much groundwork to be completed. If pursued, the finished product may well provide greater understanding of the forces which bind together sequences of action as well as those which threaten their existence.

SPECIAL REPORTS

RONALD REAGAN'S ATTEMPT TO BUILD A NATIONAL MAJORITY

Craig R. Smith

Conservatism is generally perceived as an anti-reform *status quo* political philosophy. A perceptible swing to conservativism began in 1968 as middle America sought peace through disengagement from Southeast Asia, and security through law and order at home. The trend continued during the nation's Bicentennial; 49% of the public said that they "would prefer to see the country move in a more conservative direction."[1] In the same poll only 19% preferred "a liberal direction." One manifestation of this sentiment was that no candidate surviving the first round of the 1976 primary campaign called himself a liberal.[2] One could argue that the 1976 election was a battle for the conservative vote.

This study concentrates on the candidacy of Ronald Reagan, perhaps the most conservative of the 1976 contenders.

In 1964 as a successful campaigner for Goldwater, Reagan began to consider running for public office. He seemed to realize that he had to adapt to conservatives in such a way so as to avoid alienating middle Americans. He was successful in his first attempt: the governorship of California in 1966. He was elected by a margin of 993,739 votes in a state where Democratic voters outnumber Republicans three to two. He was re-elected by another landslide in 1970. In 1976, he ran for President, and did remarkably well against the incumbent, Gerald Ford. In fact, when the votes were totaled from all the primaries, *Reagan won a clear majority*. In short, whether selling the Panama Canal as an issue in Texas, or introducing exaggerated stories in Indiana about welfare cheaters, Reagan proved remarkably effective. As we shall see, the reasons for this success are complicated, and must be qualified to some degree. But whether winning primaries or the governorship of California, Reagan was able to coalesce normally disparate conservative support.

This study will concentrate on Reagan's two major speeches: "A Creative Society," which served as the foundation for his 1966 campaign, and the March 31, 1976 Address, which saved Reagan's faltering presidential campaign. The study will compare Reagan's gubernatorial campaign in 1966 with his run for the Presidency in 1976 since both campaigns

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[1] Louis Harris, "Poll Shows Conservative Trend in U.S." *The San Diego Union* (Jan. 6, 1976) A-10. See also George Gallup, "The Gallup Poll: '76 Voters Titled to Right" *Washington Post* (May 23, 1976) A-4.

[2] Morris Udall dropped the label after the New Hampshire Primary.

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provide an excellent study of the conservative candidate who attempts to broaden his appeal to include enough voters to produce victory.

Even before he ran for governor of California Reagan was identified with conservatism. He had travelled the country and appeared on television in the cause of anti-communism. The 4,200 hours of speaking he did before more than 25,000 workers for the General Electric Company not only prepared him well for campaigning, but established him as one of big business' favorite celebrities. Even earlier, as President of Screen Actors' Guild, Reagan had appeared before a congressional hearing to denounce communism. In 1952, he spoke for "Democrats for Eisenhower"; in 1960, he gave 200 speeches endorsing Richard Nixon for President. As governor of California, he reinforced his conservative image with budget cuts, tax rebates, and anti-leftist rhetoric.

The major factor contributing to Reagan's victory in California in 1966 and in the primary campaigns of 1976 was his ability to persuade conservatives to support him. This audience made Reagan's landslide in the 1966 California gubernatorial primary possible, thus converting him into a serious candidate for the general election. Once he combined that support with middle American voters in California, he was assured of the governorship, which in turn gave him enough credibility to attempt his party's nomination for President.[3] Middle Americans both in California and in the nation as a whole were voting more conservatively in each election from 1966 to 1976. (See Intro.) Thus, the rhetoric of a conservative candidate naturally appealed to more and more voters. Of course, Reagan's quest for the presidency differed from his quest for the governorship in that he faced a conservative Republican incumbant in 1976, instead of a liberal Democratic incumbent as he had in 1966. President Ford pre-empted much of Reagan's natural constituency. As Reagan tried to communicate differences between himself and the President, he increasingly alienated moderate Republicans, not to mention the national audience. As he tried to appease moderates, he alienated more reactionary elements in the Republican party, and appeared "political." This gambit finally collapsed when Reagan, realizing that he was on the verge of losing the nomination, announced his selection of Senator Richard Schweiker for his running mate.

I will compare Reagan's 1966 and 1976 rhetoric, and then examine election returns to confirm my thesis that Reagan's success in 1966 and near miss in 1976 was due to careful adaptation to the conservative audience. This comparative methodology is particularly useful in Reagan's case since he tended in both 1966 and 1976 to rely on a "single speech." The full speech contained Reagan's carefully worked-out positions on all issues. Though he rarely gave the full speech, each segment was drawn from it and could be expanded to ninety minutes by adding the other segments.

In 1966 Reagan defined society in much the same way a philosophical conservative might: as a functional operation of various institutions.[4] For ex-

[3] Governor Brown did have his problems: he was plagued by the Chessman Case, had begun to overstay his welcome, and had lost the sympathy of some of the press, an advantage he enjoyed against Nixon. Nonetheless, given the registration figures, and the fact that he was an incumbent, Brown's loss was stunning.

[4] Craig R. Smith, *A Rhetorical Study of the Campaign Speaking of Ronald Reagan, 1964-1966*, Unpublished Master's Thesis (City University of New York, Queens, 1967). The thesis contains a large bibliography of research into conservatism as a political movement.

ample, Reagan's 1966 rhetoric was full of allusions to societal roots: "A creative society . . . experience . . . Down through the years . . . I am a Republican in the tradition of the party . . . I haven't changed my positions at all . . . history reveals."[5] The same was true in 1976 when he said, "Laws passed by Congress can be repealed by Congress, and if Congress is unwilling to do this then isn't it time we elected a Congress that will."[6] He believed in 1966 that certain values are necessary for the preservation of the societal whole: "You can't be a little bit Socialist and you can't be partly free." In 1976 he put it this way: "The Americans who keep this country going . . . are wondering . . . if the governmental institutions they have upheld and defended really are about them or their values."[7]

Reagan agreed with conservatives in 1966 that government should preserve society and its functional institutions by moral guidance: "There are . . . morals and ethics . . . that must be enforced."[8] He was no different in 1976. The "arrogance of officialdom" had neglected the need for government and its leaders to provide moral examples, "to make this land the shining golden hope God intended it to be." Both in 1976 and 1966, Reagan tried to avoid being divisive. In 1966 he initiated the "11th Commandment:" "Thou shalt not speak badly about one's opponent." And in 1976 Reagan did better in primaries where he was not perceived as splitting the party. In Indiana, for example, where he upset Ford, voters told CBS News by 4 to 1 margin that they did *not* believe Reagan's campaign was divisive. The ratio was lower in states that Reagan lost.[9] But in 1976 this strategy came unhinged as Reagan was more and more coerced to differentiate himself from Ford. By convention time, Reagan was perceived as not only divisive but hostile.

Little is known about Reagan's precampaign strategy. But again parallels between 1966 and 1976 are apparent. Reagan's speech for Goldwater in 1964 convinced many California conservatives that he could appeal to many voters. In the spring of 1965, Reagan began a state-wide speaking campaign; he impressed many Republican leaders.[10] The object was to test appeals so that those that proved ineffective could be eliminated and those that worked well could be kept. From this pre-primary campaign, positions were worked out on the major issues. Reagan was able to maintain a consistent stand on virtually every question from January to November in the 1966 campaign. Reagan's speaking tours of 1974-75 served the same purpose for his 1976 drive.

But unlike 1966, Reagan had trouble convincing members of his party that he could win in 1976. In polls comparing whether Ford or Reagan would do better against Carter, Ford came out ahead. Reagan had to rely on the primaries to demonstrate his vote-getting

[5] Ronald Reagan, "A Creative Society," Los Angeles (April 19, 1966) from the author's transcript.

[6] All 1976 excerpts are from Reagan's March 31, 1976 "Address to Nation" unless otherwise noted. This speech proved as effective as Reagan's speech for Goldwater in 1964. The 1976 effort drew an extraordinarily large audience for a political, televised speech. See "Speech Buoys Reagan Aides," *Washington Post* (April 2, 1976) A-8. The Speech earned more than a million dollars, making it the most successful single fund-raising political speech in American history. That Reagan went on to win the next five primaries is of no small consequence.

[7] "Reagan, Ford Begin Offensive," *Wall Street Journal* (July 7, 1976) 5.

[8] Ronald Reagan, "A Plan for Action," Los Angeles (January 5, 1966) from author's transcript.

[9] "Indiana Indicates GOP Split Grows," *New York Times* (May 6, 1976) 42.

[10] This operation was conducted by Spencer-Roberts Associates. (Stuart Spencer ran President Ford's campaign in 1976). Few of the speeches given by Reagan before his announcement of candidacy (January 4, 1966) were covered by the news media, nor would Spencer allow any copies to be made of their tapes.

power. In the early primaries, Reagan did much better than most challengers of an incumbent; he narrowly lost in New Hampshire (1,500 votes) and Florida. But since his local campaign managers had predicted victory, the media portrayed the Reagan performance as a failure.

To compensate, Reagan gave a half-hour television speech on March 31, 1976, which turned his campaign around.[11] He won substantial victories in Alabama, Georgia, Texas, Indiana, and Nebraska, following the speech. Each victory gave further impetus to the belief that Reagan was a winner, thereby helping him capture more votes. In Indiana, where Reagan upset Ford, voters indicated that they thought Reagan could win the national election with greater ease than Ford, a perception not shared by voters in primaries that Reagan lost.[12]

An analysis of Reagan's messages in 1966 and 1976 shows that both were essentially adapted to the same audience. He defended state and local autonomy in 1966.[13] In 1976, there was little change: "I am speaking of those programs which logically should be run at the state and local level. Welfare is an example."[14] In 1966, he depicted the Brown administration as a weak, vacillating pawn of Lyndon Johnson.[15] In 1976, he proved even tougher: "Unfortunately, Washington doesn't feel the same pain from inflation that you and I do." Later in the same address he said, "Peace does not come from weakness or from retreat. It comes from the restoration of American military superiority." In the Texas and Indiana primaries, he assured voters that "no cheap, tin horn dictator" would take the Panama Canal from him. Often, this conservative rhetoric was intensified by portraying the Russian threat in symbolic terms: "The Russians feel that they are more virile."[16]

Along with tough rhetoric came the soothing simplicity that would comfort the conservative voter who thought things were moving too fast. Reagan advised in 1966, "There are no easy answers but there are simple answers." His proposals in 1966 were often phrased in concrete, vivid terminology: "If I have here a four-inch stack of thousand-dollar bills . . . I would have a million dollars. But if we had the budget piled up . . . the pile would be more than one thousand five hundred feet high."[17] The same example was used in 1976. This "simple answer" rhetoric was collected in Reagan's "Creative Society" Address in 1966, and pervaded his campaign to "clean out" Washington in 1976.

The Protestant ethic forms a foundation for the value structures of many Americans; this is especially true of the right. During both the 1966 and 1976 campaigns, Reagan was careful to pay attention to this foundation. On *Meet the Press* in 1966 he spoke to moral values:

> I think there are certain benchmarks . . . and one of those is nothing must be allowed to take place on the campus that interferes with the primary purpose of providing an education

11 Based on reports of three networks (ABC, NBC, CBS) of primary election results.

12 "Indiana Indicates . . . ," 42. "Now, The Republican Rumble," *Time* (May 17, 1976) 10-11; "Texas Sends Anti-Washington Message," *Washington Post* (May 3, 1976) A4.

13 Charles Killingsworth, "Reagan Answers the Press" *Orange County Illustrated* (May, 1966) 3.

14 Aside from the March 31 Address, see "Where Reagan Stands," *U.S. News and World Report* (May 31, 1976) 21.

15 Killingsworth, 3.

16 "Where Reagan Stands," 20.

17 Ronald Reagan, "Budget Speech," San Diego (February 25, 1966) from the author's transcript.

. . . there have been violations of good taste, forced upon people.[18]

In 1976, the emphasis was on the work ethic: "To me there is no greater tragedy than a bread winner with a job skill willing to work but unable to find a market for that job skill." Welfare reform was simply a symbol for the value: "We have some wonderful examples here in Watts . . . of what can be done by people in cooperation with government . . . I am speaking of the work of industrialists . . . who went into Watts . . . and to date they have provided thousands of jobs."[19] In 1976 Reagan coupled these appeals with his call for the states and the federal government to pass amendments which would put a ceiling on tax rates.

Of course, a favorite theme was Washington's cronyism: "The Washington establishment is not the solution; it's the problem . . . In my own mind in California as governor I was a citizen representing my fellow citizens against the institution of government. I turned to the people, not the politicians for help." Reagan presented a strong, simple, dynamic image. However, in 1976, Reagan sharpened his image in order to contrast himself with Ford. The most prominent examples were the call for a 90 billion dollar budget cut, and for selling the T.V.A.

A final component of Reagan's conservative rhetoric was his relationship to the roots of his audience. The conversion from New Deal liberal to modern conservative served the purpose. Reagan, who "suffered through the dark days of the depression," campaigned for Roosevelt in 1932. His conversion came as a reaction to Communist threats and as a result of his speaking: "I would go out and make speeches and come back to the realization that I would turn around and vote for the people who were responsible for the things I criticized."[20] His mid-western background was also used to enhance the identification of speaker and audience.

For conservatives Reagan was able to articulate most positions in a manner not only attractive to them but to the larger audience. For example, Reagan's position on open housing in 1966 appealed to both conservatives and the majority: "My objections to so-called fair housing . . . are my concern and I think it was the concern of the overwhelming majority of people who voted for its repeal, the concern that this is a violation of a constitutional principle."[21] In 1976 Reagan was forced to be more conservative than he had been in 1966. The specificity required often led to strategic errors as in the case of the budget cut and the T.V.A. sale.

Other means of identification such as humor, drama, naming a common enemy, and using contrast conceptions were common to Reagan's rhetoric. In his question and answer periods, he assumed the role of a man on trial. Lively and often hostile questions reinforced the image. But these strategies have been studied,[22] and are not unique to the conservative's rhetorical situation. Clearly, Reagan's greatest asset was his ability to cultivate an image with which conservative Americans could identify.

The effect of Ronald Reagan's appeal to conservatives can be shown by an analysis of voting in California in 1966, and in the primary campaigns of 1976.

[18] *Meet the Press*, (September 11, 1966) from the author's transcript.

[19] *Issues and Answers* (October 2, 1966) from the author's transcript.

[20] Press Conference at the University of California in Santa Barbara, April 19, 1966, author's transcript.

[21] *Meet the Press*.

[22] See Craig R. Smith, "Contemporary Political Speech Writing," *Southern Speech Communication Journal* (Summer, 1976) 52-67.

This analysis indicates Reagan's success at phrasing conservative positions in such a way to make them appealing to those he calls the "unsung heroes," the "forgotten Americans," that is, the vast middle in America that decides presidential elections. Reagan was able to so mold the image he projected that it not only kept conservatives united, but appealed to many non-conservative voters. The statistics examined here show that Reagan's success was more dramatic in the California election of 1966 than in the Republican primaries of 1976.[23]

In reviewing the following tables from 1966, several items stand out. The appeals isolated earlier in this report seem to correspond to important vote shifts. For example, among Protestant voters, Reagan ran seven percentage points ahead of the Nixon record in his 1962 confrontation with Edmund Brown and sixteen points in front of Goldwater's vote in his 1964 confrontation with Lyndon Johnson.

TABLE 1:

VOTE PROFILE ANALYSIS OF ETHNIC GROUPS

Description	*Vote Received*	*Gain Over 1962*	*Over 1964*
Roman Catholic	45%	+4	+21
Protestant	68%	+7	+16
Latin	34%	+4	+14

More important was Reagan's performance among voters as they are affiliated with their parties. The drawing of conservative votes from normally Democratic areas can be explained by the cross-party appeal of Reagan's rhetoric. The growth of conservatism in both parties, and commonality of background Reagan shared with Democrats, helped him run well ahead of former Republican candidates.

[23] All statistics from CBS News election coverage. The author served as a researcher and newswriter for CBS News in 1968 and 1972, and as a consultant to CBS in 1974.

TABLE 2:

PARTY AFFILIATION VOTE ANALYSIS

Description	*Vote Received*	*Gain Over 1962*	*Over 1964*
Very Democratic	32%	+ 4	+11
Democratic	57%	+11	+21
Middle	59%	+ 6	+17
Republican	64%	+ 4	+15
Very Republican	73%	+ 0	+12

A county-by-county vote breakdown further verifies Reagan's conservative vote-getting power.[24] In Orange and San Diego counties, where ultra-conservatism is most rampant, Reagan ran seven percent ahead of Nixon's 1962 race, and thirteen percent ahead of Goldwater, and in moderate bay area counties, Reagan ran well ahead of Goldwater, indicating Reagan's ability to attract moderate conservatives.

TABLE 3:

COUNTY VOTE ANALYSIS

Description	*Vote Received*	*Gain Over 1962*	*Over 1964*
Los Angeles	56%	+4	+13
Orange-San Diego	68%	+7	+13
Bay Area	49%	+2	+14

An analysis of CBS News' primary election data by this writer confirmed Reagan's difficulties in 1976. While he ran ahead of the President by 300,000 votes when all primaries were counted, he did not score so impressive a series of victories as to force the President from contention. In all contested primaries, as expected, Reagan carried the precincts identified by CBS as "most

[24] This particular chart is the most useful in dealing with the tricky business of explaining Reagan's success. While attractiveness, a change in the issues, or a myriad of other factors help explain Reagan's election, none of them can explain why Reagan does so well in particular areas. These factors tend to cut across lines and geographic areas. Only conservative rhetoric, polished and modified by the Reagan team, seems to explain these statistics satisfactory.

conservative"; Ford tended to carry the precincts identified as "moderate." Other surveys showed that Reagan delegates tended to value issues, whereas Ford delegates tended to value winning.[25] This phenomenon favored Reagan since his rhetoric was issue-oriented and those voting in primaries for delegates also tended to be issue-oriented. For example, "voters who said they preferred a smaller government providing less services tended to" support Reagan; those preferring "to retain a Republican in the White House" supported Ford.[26] Most voters in Indiana favored the former position, and were issue-oriented, so Reagan won their primary. Reagan was winning delegates who were more ideological; Ford was winning the more pragmatic.

Reagan carried the states where Republicans heavily identified themselves as "conservatives." Reagan captured all of the delegates in Alabama, Georgia, and Texas running 2 to 1 ahead of the President in each of these primaries. Reagan won the Indiana primary where 43% of the voters said they were "conservatives." He lost in Illinois and Florida where only 21% and 29% respectively so identified themselves. To look at it another way, 48% of Reagan's vote in Wisconsin was composed of self-identified conservatives, 35% of moderates, and 10% of liberals; the respective figures for Ford were 35% conservative, 42% moderate and 12% liberal.[27] Clearly, Ford and Reagan cut into one another's natural strength, with Reagan winning a plurality of voters and Ford winning a plurality of delegates.

This analysis of election results verifies the notion that Reagan was able to convince conservatives that he met their criteria.[28] Examination of his 1976 rhetoric reveals that it was remarkably similar to the persuasive effort of 1966. For example, Reagan demonstrated his interparty appeal by carrying most of the "cross over vote." The difference was that in 1976, Reagan was forced to sharpen his appeals in order to differentiate them from Ford's. And in so doing, he was forced to sharpen his image and focus his program through the primary season. The result was a splintering of the convervative audience, and a loss of the party's nomination.

[25] "Reagan's Delegates Value Issues," *Washington Post* (August 17, 1976) A10. See also, "Indiana Indicates G.O.P. Split Grows," 42; W. Lance Bennett, "The Retualistic and Pragmatic Bases of Political Campaign Discourse," *Quarterly Journal of Speech* 63 (October, 1977) 237.

[26] "Indiana Indicates G.O.P. Split Grows," 42.

[27] The remainder in each case refused to identify themselves: "Composition of Candidate Support in Wisconsin Primary," *New York Times* (April 8, 1976) 31. See also, "Indiana Indicates G.O.P. Split Grows," 42. Ford carried the moderate vote by a margin of 3 to 2; Reagan carried the conservative vote 3 to 2.

[28] Reagan continued to campaign for his "philosophy" through election day, 1976. His rhetoric remained consistently conservative and effective. See "Reagan's Passionate Conservatism," *Washington Post* (October 23, 1976) A6.

FORUM

POLITICAL REALITY AND RHETORICAL CRITICISM

Dan F. Hahn and Ruth M. Gonchar

The 60's and 70's have been years of increasing sophistication in the analysis of political rhetoric. Improvements in movement studies, the emergence of genre analysis and fantasy theme analysis, the application of structuralism to rhetoric and the continued growth of Burkeian methodologies have supplied new excitement to a discipline that had high-centered on the neo-Aristotelian approach. Thus, the 1976 Speech Communication Association convention, with the theme of "Check-Up," became more a time for celebrating new approaches than reassessing those already in operation.

Nevertheless, it does seem to us that some reassessment is necessary. As we have gone about our teaching and research we have encountered three major areas where the practices of the field seem not to coincide with political reality as we perceive it: (1) the purpose(s) of the candidate, (2) the relationship(s) between candidates and audiences, and (3) the desirability of objectivity.[1]

The Candidate's Purpose

In the past, criticism has been based on the assumption that the query "What were the candidate's goals and did he achieve them through his rhetoric?" was a relevant question. But it has begun to appear that the question as stated may be irrelevant. The candidate's ultimate goal, without many exceptions, is to convince the audience to vote for him. But too often rhetorical critics lose sight of the candidate's goal—getting votes—and criticize him for not fulfilling the goal critics have chosen for him—voter education. Rhetoricians condemned Richard Nixon's "secret plan for ending the war" in 1968 because it was "purposely ambiguous"—it gained him votes but did not educate the voter. And yet, in retrospect, we notice that critics condemned the ambiguity without investigating the functions it fulfilled. The critical process was short-circuited by the flat denial of the possibility of legitimate functions for that ambiguous statement.

If it had been discovered, for example, through research, interview, analysis, that Richard Nixon's ambiguity regarding the solution of the Vietnam War functioned in at least two other ways, to promote the unhampered continuation, during the campaign, of Lyndon Johnson's peace efforts, and to allow Nixon a flexible hand once in office, the conclusions drawn about that 1968 campaign tactic might have been different. The functions discovered, the ambiguity might have been understood, tolerated, and even applauded. But the *possibility* of discovery was precluded by a belief

[1] We make no claim that these are the most significant deficits of contemporary criticism. Our purpose is not to supply a comprehensive critique of the field, but merely to highlight several problems we have encountered.

that the primary goal of political campaign rhetoric ought to be voter education. The concommitant critical judgment—that the "secret solution to the war" statement was illegitimately ambiguous—derived from that preclusion.

The belief in voter education as the goal of political campaign rhetoric has directed critics in the past to analyze a candidate's stand on the issues. What issues a candidate chooses and how he stands on them, however, seem much less important today, once the problems with the issues approach are identified. Consider the problems seen from the perspectives of the politician, the political scientist, and the rhetorical critic:

The problems as identified by Boston Mayor Kevin White:

> It would be great if they handed you a checklist of problems and after you were elected you could go down the list bing-bing-bing and take care of all the problems. But that's not what happens. I had no way of knowing that two months after I took office as Mayor of Boston, that the biggest problem I would face would be getting fuel oil into the city to keep the people warm. That's not the kind of thing you anticipate.[2]

But it is the kind of thing a government official has to deal with, regardless of the lofty issues discussed in the campaign.

The problems as identified by political scientist W. Lance Bennett:

> Most issues in most campaigns display the following characteristics: (a) they are seldom defined to the degree necessary to debate clear policies or programs that might derive from them, (b) they are generally linked to familiar characterizations of the candidates and the parties rather than to conditions and causes that fall beyond the scope of the election, (c) they may be defined by the candidates in different terms for different audiences, (d) they are seldom defined in terms that will arouse controversy or that will risk damage to the candidate's public image, (e) they are often linked to claims about the personal qualities of the opponent or to propositions about leadership, and (f) they are usually restricted to a fairly small range of substantive concerns.[3]

The problems as identified by rhetorical critics Dan Hahn and Ruth Gonchar:

> The candidate may not be saying what he really believes; he may have stated his "position" in such a vague way that we are led to think he argrees with us when he really does not; he may change his mind after he is elected.[4]

In short, the issues chosen by the candidate and the stands he takes on them may not provide the rhetorical critic with many clues about the orientation of the candidate toward issues which may surface during his term of office. So the reassessment here requires the realization that in more instances than not the issues are transitory and, thus, rhetorical analysis must use the words of the candidate to evaluate more than simply "where he stands" on those issues.

Several approaches are available for this effort. First, the critic can examine the candidate's assumptions about his audience by the way in which his rhetoric develops the image he is trying to portray. For example, if he projects a father image he may assume the citizens are mere children—and be tempted to treat them as such once in office. Now obviously no candidate straightforwardly asserts, "I am the father, you are the kids." But a careful critic can sometimes find that attitude reflected in the candidate's rhetoric. For instance, when Nixon said, on September 19, 1968, "Let

[2] Quoted in Joseph Napolitan, *The Election Game and How to Win It*. Garden City: Doubleday and Co., Inc., 1972, p. 129.

[3] W. Lance Bennett, "The Ritualistic and Pragmatic Bases of Political Campaign Discourse," *Quarterly Journal of Speech*, 63 (Oct., 1977) 220-221.

[4] Dan F. Hahn and Ruth M. Gonchar, "Political Myth: The Image and the Issue," *Today's Speech*, 20 (Summer, 1972) 58.

me be very clear about this: The next President must take an activist view of his office. He must articulate the nation's values, define its goals and marshall its will,"[5] he certainly identified the President as the omnipotent father, leaving less autonomy to the electorate than the average father allows his 15-year-old.

Second, the critic can investigate the way in which the candidate describes the office he is seeking. Whether he has an expansionary or stewardship view of the office sought indicates a great deal about how he would approach his job if elected. Rhetorical critics in 1960, for example, should have been aware of these Kennedy lines from a 1957 Senate debate: "The President of the United States, if he felt the national interest were at stake, could take the United States into war without consent of the Congress, although he would subsequently be obliged to come to the Congress for approval. But there is nothing that lessens the authority of the President to use United States forces if he thinks the best interests of the United States are at stake."[6] This one paragraph is more revealing than all of the campaign debate about Quemoy, Matsu, Cuba and the mysterious Missile Gap combined.

Third, the critic can explore the way in which the candidate defines political situations. Does he, for example, like Richard Nixon, see situations in crisis/non-crisis terms. Does he, like Carter, depend upon "fine semantic distinctions" to reconcile conflicting positions? For example, "When, after publicly declaring that he never asked for political endorsements, he sought and gained Birch Bayh's backing in the Indiana Presidential primary, Carter explained; 'I asked for his support, I didn't talk about an endorsement.' "[7] Does he, like President Ford, knowingly mis-define for political purposes? Ford defined the capture of the Mayaguez as an act of "piracy" despite the fact that his legal counsel had assured him that Cambodia's seizure of that ship was, under the circumstances, perfectly legal under international law.[8] Clearly, how a politician approaches a problem will be determined by how he defines it—and if one can detect a pattern in the definitions, the insight provided into how the speaker's mind works is much more valuable than the discovery of his position on any of the transient issues lying about when the calendar anounces it is time for another election.

Fourth, the critic can learn about the candidate from his "linguistic footprints,"—metaphors, adjective-noun ratios, type-token ratios, etc.—those portions of a speech not easily controlled by the candidate. Assuming that Burke was right when he contended that people have a "kind of terministic compulsion to carry out the implications of their terminology,"[9] then there may be clear links between a politician's "linguistic footprints" and his political solutions. For instance, a careful analysis of the famous Wheeling, West Virginia speech of Senator Joe McCarthy shows that his factive and cleft sentence constructions allowed him to assume the existence of a conspiracy without having to prove it,[10] in much the same way that the old cliché about whether a person is still

[5] Quoted in Lewis J. Paper, *The Promise and the Performance; The Leadership of John F. Kennedy*. New York: Crown Publishers, Inc., 1975, pp. 117-118.

[6] Quoted in *Ibid.*, p. 64.

[7] Robert Shogan. *Promises to Keep; Carter's First Hundred Days*. New York: Thomas Y Crowell Co., 1977, p. 42.

[8] Jack Anderson, "Veterans' Benefits," *New York Post*, May 20, 1975, p. 41.

[9] Kenneth Burke. *Language as Symbolic Action; Essays on Life, Literature, and Method*. Berkeley: University of California Press, 1966, p. 19.

[10] We are beholden to the late Professor Beth Susman of Alvernia College for this example.

beating his wife assumes what needs to be proven. Other "linguistic footprints" have been noted in Edwin Black's study of the Communism-as-cancer metaphor[11] and Thomas Benson's analysis of the literature-as-poison analogy.[12]

These, then, are some of the alternatives to the straightforward analysis-of-the-issues aproach, choices which are less likely to result in futile complaints about how the candidate is being ambiguous when he should be educating the voters.

Candidate-Audience Relationships

A second area that needs to be reassessed is the way in which we investigate speaker-audience relationships. When rhetorical critics began to abandon the speaker-persuading-the-audience linear approach to the speaking situation, the Burkeian concept of "identification" came to govern critical investigations. But the reality of a political audience is often different from either the linear approach or the process whereby a speaker identifies with the characteristics of his audience. The political speaker (1) frequently finds a "safe" audience to whom to deliver a controversial speech (Nixon at Kansas State), or (2) writes a speech merely to get on record, in which case the immediate audience is largely irrelevant (Churchill at Fulton). These points are already known to most rhetorical critics. However, critics also have begun to recognize that identification may not always be with the content of the speech but with a component of a candidate's image. For example, a political candidate's support of capital punishment may work to reinforce, not the audience's identification with the issue (and thus with him), but the candidate's machismo image. In this respect, the stand a candidate takes on an issue acts to mediate identification between audiences and candidate image. One result is that the audience can identify positively with the candidate's image without necessarily agreeing with his stand on the issue. When Lindsay took a pro-busing stand in the Florida primary in 1972, it was to reinforce his "a fighter, not a talker" theme. He didn't expect his audience to agree with him on the issue, but to respect him for having the guts to stand up for his beliefs.

It should also be noted that, implicit in the Burkeian concept of identification, there is an assumption that identification is always positive. Except in instances where *ethos* demands expertise rather than similarity, identification is assumed a benefit rather than a hindrance to persuasion. Not so, says the political professional who understands the process of "negative identification." An audience perceives a characteristic in a speaker that they are forced to recognize in themselves—for example, an ethnic orientation. They dislike the characteristic in themselves and they are distressed that the candidate forces them to remember the characteristic. They identify negatively with him and vote for his opponent. Abraham D. Beame, a liberal Jewish Brooklyn Democrat, did not received the support of a majority of Jewish voters in New York City when he ran for Mayor in 1965, even though Jewish voters traditionally support liberal Jewish Democratic candidates. Evidence indicates that they identified negatively with Beame—they did not want the first Jewish Mayor of New York City to look and sound like Mr. Beame.[13]

[11] Edwin Black, "The Second Persona," *Quarterly Journal of Speech*, 56 (April, 1070) 109-110.

[12] Thomas W. Benson, "Poisoned Minds," *Southern Speech Journal*, 34 (Fall, 1968) 54-60.

[13] Ruth M. Gonchar, "Common Ground: Positive and Negative Identification," *Speaker and Gavel*, 20 (Summer, 1970) 16-21. The concept of "negative identification" evolves out of Ruth M. Gonchar, "William F. Buckley, Jr. and the 1965 New York City Mayoral Race," unpublished M.A. Thesis, Temple University, 1966.

A more recent example from New York City concerns the declining career of Bella Abzug. It seems clear that New Yorkers have come to feel that she too perfectly epitomizes the brashness of New York City. They have come to identify her negatively, so "Bellicose Bella" has lost her last three tries for elective office.

It should also be noted that something very like negative identification happened to President Ford in terms of his clumsiness. Despite the fact that we have all bumped our heads on occasion, or stumbled on a staircase, we did not identify positively with Ford when he suffered these very human accidents. But in this case there was the additional burden of the Johnson characterization of Ford as a person who couldn't walk and chew gum at the same time, a characterization which was taken to apply to the intellectual as well as the physical realm. So much was Ford haunted by this phrase that when he pardoned Nixon the standing joke in the Senate cloakroom was that Ford had taken to chewing gum again.[14] Whether and to what degree these relatively trivial incidents and jokes affected the 1976 outcome can not be determined—but certainly the negative identification, whatever its degree, did not help him.

Objectivity in Criticism

The final reassessment to be considered here revolves around the need for objectivity. And this last check-up involves both the critics and the audience to whom criticisms are directed. Critics generally understand that they cannot be totally objective: indeed, the idea that any criticism can be objective must tickle the funnybone of a dozen contemporary philosophers. Yet some of the journals of our discipline demand that critics write as though they were objective. Accordingly, (1) everybody realizes that no one can be objective, yet (2) critics are not allowed to state their prejudices so that the reader may understand through what emotional filters the rhetorical criticisms have passed, thus (3) critics are not allowed to make value judgments unless they are cloaked in neutral words. It is unacceptable to say that a political candidate lied; the critic must say that he "seemed to be dissembling" or he was "less than candid." The result often is that rhetorical criticisms are less than candid—and boring as hell.

Recently, one of the authors of this article wrote a piece on President Ford's handling of the Mayaguez affair and proposed that it was an example of corrupt discourse—for four violations of the canons of ethical rhetoric. The article was returned as unacceptable with these remarks: The author argues . . . that 'the government willingly lied to us.' This is a harshly prejudicial and unscholarly report. Such polemical outbursts are more appropriate in letters to the editor or journals of opinion." Further, the reviewer suggested that the author wait till Henry Kissinger published his memoirs before attempting to assess what *really* happened.

The point is not that the manuscript should have been published—maybe it wasn't good enough. But it should not have been rejected for the reasons given. It is not prejudicial or unscholarly to arrive at conclusions based upon evidence, regardless of how strongly worded such conclusions may be. There *may* have been some bias in the piece—but there was even more bias in the editorial judgment that no conclusion of that nature could possibly be arrived at without the concurrence of a Henry Kissinger. To pretend that criticism is (or should be) unbiased and that, therefore, our

[14] Paper, *op. cit.*, p. 198.

speech communication journals are not "journals of opinion" is to go all the way beyond myopia to blindness. And if "bias" equals "unscholarly" then it is doubtful that there has ever been a communications scholar. As Murray Edelman has noted, the attempt to conceal our preferences only produces ". . . a *dramaturgy* of objectivity, a stand that has contaminated too many studies, discouraging the self-critical and tentative posture that investigators and critics need to maintain."[15]

What is being suggested then is that it would make more sense to admit critical biases, as openly as possible, and let the reader see more clearly the forces that informed rhetorical conclusions. In that way, perhaps rhetorical critics (as well as the citizens they train) would be allowed and encouraged to critique contemporary rhetoric without waiting for Henry Kissinger's memoirs. It needn't be instant or querulous, but it does need to be in the same frame as the rhetoric analyzed if it is to be anything more than a quaint literary exercise. At a time when the imperial presidency grows more and more distant from the people it is supposed to represent, critics can no longer afford the luxury of waiting until the results are already known before speaking out.

If rhetorical criticism is as tuned to political reality as we all hope it is, and if it is as important as those of us practicing it obviously believe it is, then it makes little sense to impose unrealistic restrictions upon ourselves which have the effect of making our products dated and irrelevant. Rhetorical criticism of political rhetoric will not die if we continue to insist upon objectivity, but we run the risk of setting up a situation where nobody would notice if it did.

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[15] Murray Edelman, *Political Language; Words That Succeed and Policies That Fail.* New York: Academic Press, 1977, p. 15.